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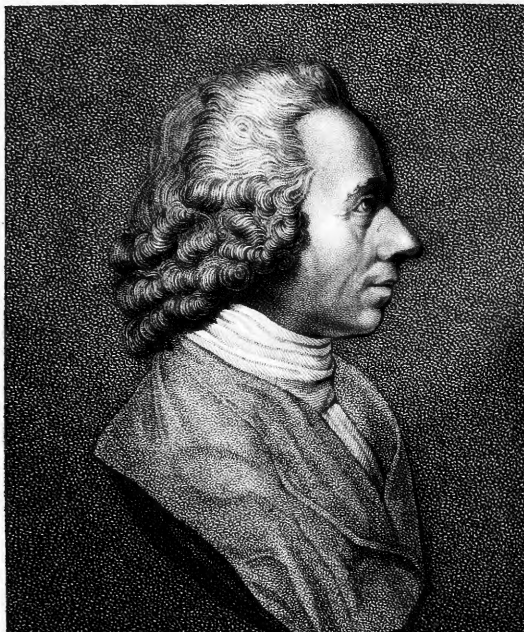

Engraved by E. Mackenzie, from a Basso Relievo Modelled from the Life.

J. Priestley, L.L.D. F.R.S.

Published July 31st 1805, by A. Tilloch, Carey Street.

THE PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* ~~lib. i. cap.~~ I.

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1806.

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THE
PHILOSOPHICAL MAGAZINE.

I. *Some original Remarks on a Variety of the Genus Acarus, belonging to the Order Aptera, found on the Wings, &c. of a feathered Fly of the Order Diptera.* By *Mr.* John Snart, *Optician.*

To the Editor of the Philosophical Magazine.

SIR,

I should not have thought myself very well employed in writing the following particulars of so trivial a creature as I am about to treat of, were it not with the view of drawing some new inferences, and even an unequivocal line of demarcation between animalcula that are of a perfect generation and those which are not so, although they are often indiscriminately blended: and though I will not peremptorily dare to dogmatically assert that the one in question is the *ne plus ultra* of nature's perfect beings, in a small way; yet it is the utmost boundary I have seen, or even read of in those entomological writers who have given simple statements of facts only. I do not mean to include the animalcula infusoria in this idea; for it is plain that nature's manner of perpetuating a species by mere vegetative impregnation is essentially different and distinct from sexual intercourse, or copulation; although it may be urged by some (what I think is very doubtful), that the germ or ova of the former is always shed or ejected in the water, &c. beforehand: but as the common pediculus, or louse, it is evident, is originally formed from the exudation of the skin, which vegetates, becomes vascular, organizes, and lastly animates, and assumes loco-motion (without a father, and is therefore *sui generis*), this also may be the case with the creature of which I am writing, which may originally be produced in this way, as well as many others; but after they have attained the capacity of propagating their species *per se*, provident nature, whose characteristics are simplicity and œconomy, and who never makes use of two sorts of means where one will answer the intended end, may leave them to shift for themselves when capable. As we take away the leading-strings when the child can walk alone;

alone; so she may choose to diversify her manner of perpetuating these species, by leaving them to the impulses she has planted in them.

It is this manner of propagation in this species, to which I have been an eye-witness, that emboldens me to think that this creature, though otherwise insignificant, is entitled to some distinction in that part of natural history called *entomology*; and that from this source, it, being so small, though perfect, derives all its consequence.

I think it is pretty obvious that this secret diversity of our grand parent, from whose inexhaustible source we are continually extracting fresh information, is the cause of different opinions in various writers upon the several branches of natural philosophy; because, if one man should see a creature produced by vegetative impregnation only, which another knows to be produced by sexual commerce, and each should be ignorant of the other means, they will reciprocally contradict each other; while a knowledge of both harmonizes the whole, without hypothesis or concession; and in several species of insects one male impregnation will pass through, or serve for, several generations. The creature before us is one of the multitude of examples which proves that our common parent is no less wonderful, but much more profuse, in her smaller than in her greater productions; which affords a presumption, that were our optics commensurate to the task of viewing her exquisite works to perfection, there may exist a part of her diversified chain of animated beings as much more minute than the animalcula we are familiar with by means of our natural organs, as they are inferior in size to the elephant, the supposed extinct species of the mammoth, or the more doubtful kraken itself.

> And though their vital spark is pent in narrow bounds,
> They feel each speculator's philosophic wounds,
> When calmly they're impal'd alive to feast his eyes,
> (Regardless of their agonizing throes and sighs,)
> And pain as much, at death, as when a giant dies!

In the instance now before us we have indubitable proof of identical existence, distinct and separate volition, with a capability of performing all the voluntary as well as involuntary functions of life, together with the œconomy of perpetuity by propagation, &c. &c.; which, in my ideas, constitute the creature in question, at least in this stage of life, as distinct from the merely impregnated existence as the animated from the vegetative creation, and therefore justly entitled to a place in the sentient list of nature's animated

mated works. And in this particular I think a natural line of demarcation between the monads of matter, organic particles, or first rudiments of future being, and distinct beings, is quite apparent.

For it is not the oscillatory motion of the semen masculinum, the attractive and repulsive motion of some of the animalcula infusoria, or the rapid vegetative growth of the mutilated parts of these semi-animated creatures called polypi, that so properly constitutes life, consciousness, and perfect being; but predilections, volition, propagation of species, &c. are primary characteristics of being: and from these criteria we ought, *à posteriori*, to draw our inferences of different degrees or modes of existence, because they are so much more satisfactory than the indefinite one of locomotion, or any other of the non-naturals.

From these particulars I infer that this creature is neither larva nor infusoria, but a perfect insect; because the one cannot propagate, and the other is not begotten by an identical parent. And it is plain that it differs very essentially from the animated moleculæ or organic monads, or particles of the semen masculinum, which are rather the primitive rudiments of future beings than beings themselves.

It is as far back as May 1803 since I first made the discovery of these insects; but, as I would not impertinently obtrude every trifling discovery upon the world, lest it should have been published before (and I despise plagiarism), I have let it lie by ever since, that I might inform myself of this circumstance did it exist; and by this forbearance I have discovered that there are as many as five-and-thirty varieties of this genus, known in the Linnæan catalogue, chiefly distinguished by the numbers and length of thorns, or hairs, which issue from their posterior parts. But of this variety no particular mention is made by that great man, that I can learn; and this, together with some original remarks, which my attention to the subject has enabled me to make, seems to justify the present observations.

In the year and month above mentioned I happened to catch a feathered fly of the order *diptera*, which curiosity prompted me to reserve for the microscope, with a view of examining its beautiful plumage, or feathers, with which the legs are plated or covered, and the wings striated and bordered: each wing has six of these striæ of delicate fimbriated feathers, and fimbriated with a border of the same, issuing out at an angle of about thirty degrees from their source: the other part of the wings is reticulated by veins, so as to ap-

pear like fine gauze. The whole fly about the size of the common gnat.

Upon a minute examination of the plumage, &c. I casually discovered the subject of this paper: whereupon I took a deeper magnifier, which enabled me to see that every part abounded with them, and that the one I first saw was not an accidental visitor, but that they were the common vermin with which this fly is infested; which indeed is no great wonder, when it is considered that far less creatures have some peculiar tormentor; for even that notably disgusting pestilence the louse is not exempt, but is over-run at times with his pediculi, in return for the compliments he pays to some of us *.

The object of my pursuit was now diverted from the fly and his plumage to his incumbent tormentors, which I soon discovered to be of the oviparous kind, and that some hundreds of the ova were scattered over one wing, several of which I saw drop off the parents while they perambulated their prey, which were afterwards nidified, or hatched.

I should have observed before, that the eggs of this insect are never incubated at all, but instead thereof the parent literally clothes herself with them; for by means of some kind of mucilage, which either exudes from the parent, or with which the ova are covered, they firmly adhere to her: but it is most natural to suppose this supply is from the parent, by which means the ova are brought to perfection; and as this mucus is absorbed they harden and fall off; after which, in a few days, a development takes place. And here the agency of the sun is of essential service, as it not only very much accelerates their maturity, but also stimulates the full grown to fulfil the great purpose of nature, which they, like the canine breed, perform posteriorwise. But I never saw the least propensity to this act until they were placed in that situation, or any of the eggs hatched anterior to this: but all their motions were slow and languid heretofore; whereas now they became reanimated, with the greatest promptitude imaginable, running about with amazing celerity, &c. The rotundity and semi-transparency of their bodies gave them the appearance of moving prolate spheroids of moistened ichthyocolla, and quite as clear throughout; for, like all insects, their blood is as limpid as distilled water, so that there is no impediment to the sight. And as the microscope I applied them to magnified several thou-

* Thus a distinct species of pulex, gorged with a serrated collar, is found on the mouse:—and all creatures have some kind of vermin.

sand

sand times, they appeared as large as walnuts, and I could distinguish every part with ease. The ova, or eggs, were also seen as large as kidney beans, and looked like polished mother-of-pearl; and, owing to the solar rays, as finely variegated, with all that soft delicacy of prismatic tint so peculiar to that shell.

The legs of these insects, which are eight in number, are so small in proportion to the bulk of their bodies, that if the creatures happen to fall from any eminent part of the fly, upon which they feed, and get upon their backs, they are as unable to recover their feet as a tortoise; partly on account of their want of the vertebral joints or spines, and partly from the inconsiderable weight of their legs to form a counterpoise. But what they are deficient in in these respects is by provident nature compensated for by their ingenuity and address, as is plain by their taking advantage of any adventitious opportunity for their relief that may present itself, and which they have the sagacity to improve and make subservient to their present convenience: for, each leg being composed of four articulations terminating in a double talon, they catch hold of as many of their young as are adequate to form a sufficient weight of lever, when properly applied, to restore them; to which purpose they place them all on one side; and so completely counteract the weight of their bodies, and recover their lost position.

The facility and address with which they wield six or eight small ones (quite as easily as a man can a walking-cane) not only argues great strength of body but great ingenuity also; while the infant tribe appear to sympathize with the incumbent parent while in that supine posture; who no sooner falls than she is surrounded by these little auxiliaries, each of which places itself on some part of the parent, as if emulous to assist (and it cannot be to suck, because they are not of the mammalia class). Thus we may see in an insect, which in its first development is not more than a sixteenth part the size of a small grain of sand, and in its mature state not bigger than the grain itself, all those powers of instinctive motion that can be found in one ever so large or noble.

Of how small worth, then, are the attainments of nine-tenths of the human species, who perform a dull, unmeaning, sensual round of unconscious mechanic actions, as they feel themselves impelled by instinct, not one of which is superior to those of these beings, whom they destroy

without knowing such creatures had an existence! (I omit the black catalogue of their faults.)

Then what is life! what all its functions too!
When largely giv'n to mere ephemera!—
The animating pow'r is wondrous cheap!
Or much our parent's largess is unknown!—
Your boast (preheminence), vain man, forgo,
Or blush, and turn this gift to more account.
For if the thinking man is censur'd thus,
And e'en outrivall'd by a very mite,
What shame awaits the drowsy sensu'list,
Who eats, drinks, sleeps, and toys; then dies a fool!

If these particulars should be thought of sufficient moment to entitle them to a place in your vehicle of philosophical information, by laying them before the public you will confer another favour on, sir, your already obliged

Servant at command,
JOHN SNART.

215, Tooley-street,
August 10, 1805.

Explanation of the Plate.

Fig. 1. (Plate I.) Back view of the acarus, with the ova attached: also two young ones, showing the comparative size of them with a full grown insect.

Fig. 2. Abdominal view, showing the manner in which the legs, &c. spring from the sternum and belly: also the comparative length and usual number of hairs which issue from the posterior parts,—in general about thirteen or fourteen.

II. *On a Colour for marking the Ends of Cotton or Linen Cloths capable of resisting the Operations of bleaching, and likewise the most complicated Manufacture of printed Cloths, without extending beyond the Limits of the original Impression. By M.* HAUFFMAN *.

To obtain a colour proper for marking cloths of every kind, it is necessary that no substance or drug soluble in alkaline leys should enter into its composition. It is equally requisite that the substances intended for any composition whatever should not turn white when combined with oxygen; and that they should remain indissoluble in acids of the strength required for bleaching, as well as for the preliminary operations in the fabrication of printed cloths. Colours composed with drying oils cannot, therefore, in my opinion, be employed for this kind of marks, because they are not only liable to be attacked by alkaline and

* From the *Annales de Chimie*, No. 158.

saponaceous

saponaceous leys, but because in drying slowly they run, and very often occasion spots.

If those composed with spirit-varnish were even not attended with the inconveniences of speedy evaporation and desiccation, still it would be as improper to employ them as the preceding, because, turpentines and resins are very easily transformed into soap. Nor can gum-copal be used for colours for marking, because it is detached from the stuff by mere ebullition in water.

By employing oil of turpentine, which evaporates and dries less speedily than alcohol, I have succeeded in obtaining a black composition, which to me appeared capable of being used with advantage for marking cloths. It is made by dissolving slowly in oil of turpentine, in a sand-bath, continually stirring it, a quarter of its weight of asphaltum, or bitumen of Judea, broken into small pieces, and afterwards mixing with it as much as possible of lamp-black, or black produced by any mineral substance whatever, highly coloured, and in very fine powder; either carburet of iron, sulphuret of lead, or any other. The colour will be obtained more or less thick according to the proportions of the oil of turpentine and bitumen; it will mark exceedingly well without running, observing the just proportions, and diluting it with a new portion of oil of turpentine, if while it is in use it acquires too much consistency. This bituminous colour bears equally well the action of alkaline leys and of oxygen, and resists all acids of a certain strength.

Finding it unnecessary to continue the trials of oily colours, I undertook the aqueous experiments in the following order:

Exp. I. I dissolved in four ounces of water one ounce of sulphate of manganese, deprived of its water of crystallization, such as is obtained by procuring the oxygen gas of the black oxide of manganese, by means of sulphuric acid, and by increasing the violence of the fire towards the conclusion of the operation, so as to ignite the retort. This solution was thickened with a dram of fine gum-dragon in powder, and coloured with lamp-black, to render the accuracy of the impression more visible, which is executed very easily with this black, saline, metallic mass, of which, however, no use can be made, excepting the ends of the marked cloths be plunged into alkaline ley, without previously passing it through water, to take away the saline matters. The ley may be made with potash or soda, in the proportion of one part of alkali to from nine to twelve parts of water: it may be used in the state of carbonate, or

rendered caustic with half a part of quicklime. The precipitation of the oxide of manganese from marks, by one or the other of these alkaline leys, will take place (allowing for its coloration by the lamp black) under the colour of a yellowish white, which will gradually turn brown by the attraction of the oxygen of the atmospheric air. The alteration of these marks to brown will take place very speedily, and even with a stronger intensity, approaching to black, if you bleach by means of an alkaline oxygenated muriatic ley, the cloths, the ends of which have been plunged for a few minutes into any kind of alkaline ley whatever. These marks of brown oxide of manganese resist, not only all the processes of bleaching, and all acids of the strength required by them, but likewise the more complicated processes employed in printing cloths.

Exp. II. Had not acetic acid more affinity for manganese than for iron, and were it not disengaged as easily from acetate of manganese as from the acetic solution of iron, by evaporation and desiccation, inalterable marks might be procured in the most simple manner, by causing the oxide of manganese to adhere to stuffs by means of acetic acid, and afterwards exposing that oxide to the attraction and saturation of the oxygen of the atmospheric air. The acetic dissolution of manganese may very speedily be obtained by mixing, in suitable proportions, acetate of lead with a solution of sulphate of manganese; but as this acetic solution possesses no advantage over the sulphate of manganese for marking stuffs, as it is necessary, before it can be used, to subject it in every respect to the treatment described in Exp. I., and as it is much dearer, it is not adviseable to employ it.

Exp. III. Two ounces of sulphate of manganese dissolved in eight ounces of acetic solution of iron, concentrated to twenty degrees, furnish, when thickened with one-fortieth part of gum-dragon, a deep yellow colour, which gradually turns to a brown if treated exactly in the manner described in Exp. I. The acetic solution of iron affords for the rest no other advantage than that of causing the colour of the marks to dry rather more speedily; for the oxide of iron dissolves more or less rapidly in acids, in proportion to its state of oxygenation or oxidation. I prefer gum-dragon for thickening marking colours to the other gums and to starch, because those substances weaken the colours too much by their interposition: if, however, in the marking of coarse cloths, gum-dragon should be attended with any difficulties, it would then be necessary to have recourse to starch.

Exp.

Exp. IV. If, in the disengagement of oxygen gas from a mixture of black oxide of manganese and sulphuric acid, care be taken not to push the fire to incandescence, the saline residue is blackish; but with a violent heat it turns to a yellowish white. On dissolving this residue you separate from it, by washing, an oxide of a dark gray, which acquires the consistence of paste on the filter when deprived of the aqueous vehicle. On mixing this gray paste-like oxide with ever so small a quantity of water, thickened with gum-dragon, and printing with it, you obtain marks of an extremely dark gray, which dry very speedily. This gray colour cannot be removed by water, though it may not have been steeped in an alkaline ley: it is so tenacious and unalterable, that it withstands not only the action of all the acids of a certain strength, but likewise all the processes of bleaching, as well as the most complicated fabrication of printed stuffs, without attracting the colouring parts of any dye whatever.

Exp. V. Were it not for the apprehension of weakening a little the place where the stuffs are marked, equal parts of a mixture of the above-mentioned gray paste and a nitro-muriatic solution of tin, charged with one-fourth part of the metal, and thickened with gum-dragon, might be employed with advantage. This colour is equally unalterable with that of the preceding experiment, and it possesses the additional advantage of attracting, by its oxide of tin saturated with oxygen, the colouring parts of any dye whatever, and turning to dark brown in dyeing with madder. I shall observe on this occasion, that, by this madder-dye, the colours of marks produced by oxide of manganese, saturated with oxygen, turn to a dark brown approaching to black, whereas in a state less oxygenated they assume shades more or less different. However, in all these circumstances it is necessary that there should be as much metallic oxide as possible, without which you obtain only light tints of various other colours.

Exp. VI. Seeing that many insoluble metallic oxides nevertheless acquire the property of adhering to stuffs by means of acids, I resolved to try if the same was the case with the precipitate of manganese saturated with oxygen. For this purpose I dissolved one part of sulphate of manganese in six parts of water; and afterwards proceeding with the precipitation to the point of saturation with a caustic alkaline ley composed of half a part of quicklime, four parts of water, and one part of calcined potash of commerce, I obtained a precipitate of a yellowish white. I then added to the whole aqueous mass a sufficient quantity of oxygenated muriatic

alkaline ley till the precipitate was completely saturated with oxygen, and its brown colour ceased to increase in intensity. Then collecting on a filter the precipitate, or brown oxide of manganese, I let it stand till, by the loss of water, it assumed the consistence of paste. This brown paste mixed with half its weight of acetic acid, as highly concentrated as possible, yielded only a weak brownish tint; and it continued the same after the addition of any of the three acids, sulphuric, muriatic, and nitric, weakened with water. I obtained a result not more favourable on mixing a part of the above-mentioned brown paste with an equal portion of acetic solution of iron, marking twenty degrees on the areometer for saltpetre, and thickened with gum-dragon. This acetic solution of iron, containing only the quantity of oxygen necessary for the solution of the metal, seized, by a much stronger affinity, the excess of the oxygen of the brown oxide of manganese, which was afterwards completely dissolved in its turn; and from the whole resulted a mixture of solutions of two different metals of a reddish yellow colour, very deep, and transparent: which confirms the observation, that a saturated metal requires less acid for its solution than if it were in a contrary state; and that, being then provided with an excess of acid, this solution, saturated with oxygen, is capable of admitting a portion of another metal without being disturbed. This mixed solution of two metals yielded me only a rust yellow, which diluted sulphuric acid carried away entirely at the expiration of a time rather longer than that required to remove a less oxygenated rust yellow. To obtain from these two metallic solutions a marking colour impossible to be effaced, it was necessary to steep the marks for some minutes in an oxygenated muriatic alkaline ley, in order to precipitate, and to saturate with oxygen the oxide of manganese. By the mixture of another half part of the brown paste of manganese with two parts of the solution of the two metals, this new portion remained unaffected, and disturbed the whole. This turbid mixture being thickened, yielded only a faint brownish tint on the stuff, after remaining a considerable time in diluted sulphuric acid.

By means of the muriatic solution of tin, which possesses the property of seizing the oxygen of various substances, vegetable, animal, and mineral, and which, for this reason, may be advantageously employed in dyeing, as well as in the manufacture of printed stuffs, the darkest oxides of manganese and of iron are deprived of colour, and instantly dissolved; which demonstrates the more powerful affinity of tin for oxygen than of manganese or iron.

Note.

Note. No apprehension need be entertained respecting the effect of steeping marked cloths in an alkaline ley; it is an operation which is speedily performed without any perceptible loss of potash or of soda, if you first proceed to wash with ley, for which purpose that which is left may be again employed. If, conformably to the practice I have followed for a number of years, the alkalies for leys were made caustic with quicklime, a great quantity of soda and potash would be saved, and at the same time a superior effect would be produced.

III. *A Varnish which preserves Vessels made of Copper or other Metals from the Action of Acids of a certain Strength. By M.* HAUFFMAN *.

To obtain from copal a varnish fit for this purpose, of a whiteness and transparency resembling water, it is necessary to employ copal reduced to a very fine powder, and to expose it, with twelve parts of fine oil of turpentine, for some hours, or till it is completely dissolved, to the moderate heat of a sand-bath, in a capsule of glass, stone, or porcelain; observing to stir the whole very often with a glass rod. It is at the moment when it begins to acquire the consistence of syrup that the total dissolution of the copal takes place by means of the stirring, which is facilitated by the occasional addition of a small quantity of oil of turpentine to replace that which evaporates. Three-fourths of the oil of turpentine which are lost by the evaporation in open vessels may be saved by making the solution in a long-necked matrass, exposed to a sand-bath a sufficient time to complete the dissolution of the copal, shaking it at the same time very frequently. The varnish obtained by one or the other of these methods turns of a yellowish colour if the heat be too violent; and as its application would be difficult when its consistence too much resembles that of honey, it is adviseable, instead of diluting it with oil of turpentine, to mix it with one-fourth or one-fifth of its weight of alcohol; taking care not to put more than is necessary, for an excess would turn it to a milky white by the precipitation of part of the copal, which admits into solution with it only a certain quantity of alcohol without being precipitated. Vessels of copper, or of any other metal, may receive one, two, or three coats of this varnish, and ought each time to be thoroughly dried in an oven; after which

* From the *Annales de Chimie*, No. 158.

they

they bear extremely well to be washed with boiling water, and are capable of resisting a heat of a still more elevated temperature without losing the varnish; but at all events care should be taken not to rub the vessels with sand, or any other hard bodies.

IV. *New Method of extracting raw Sugar from the Beet-root. By M.* ACHARD*.

THE beet-roots, properly cleansed, must be subjected to the press, by which a thick juice of a dark colour will be obtained; which, besides sugar, contains also albumen, extractive matter, and other substances, which must be separated before sugar can be obtained. In this separation consists the principal part of the process.

In a tin or tinned copper boiler mix 100 pounds of the juice of the beet-root with three ounces six drams of sulphuric acid diluted in one pound of water, and immediately pour the matter into vessels, in which it must be left standing twelve, eighteen, or twenty-four hours. Twelve hours are sufficient, but it cannot sustain any injury in twenty-four, as the acid prevents any alteration of the juice. To separate the sulphuric acid, incorporate with the juice seven ounces and a half of wood ashes, and afterwards two ounces six drams and a half of lime slaked with water. The sulphuric acid coagulates the albumen, and the ashes, with the lime afterwards added, separate the acid in the form of a salt not very soluble. Indeed, it is a well known fact, that in the manufactories of raw sugar in the West Indies, as well as in our sugar-houses in Europe, lime is employed to promote the separation and crystallization of sugar.

Whis this operation has been performed, it is necessary to clarify the juice of the beet-root; for which purpose it is removed into a boiler, set in such a manner that the fire can act only on its bottom. The fire is increased to a degree approaching to ebullition without stirring the liquid. The fire is then extinguished, and the scum is taken off as fast as it rises, in the form of large black flakes. The liquor is afterwards passed through a wooden strainer, taking the precaution not to shake it too much, lest the sediment should be mixed with it, and stop the pores of the strainer. The scum and the dirt left upon the filter serve as food for pigs.

The juice, thus clarified and filtered, is poured into a boiler, with a flat bottom, to the height of only six inches,

* From *Neues Journal der Chemie.*

and

and is then evaporated with a brisk fire. By this method the juice is prevented from being converted into a mucoso-saccharine liquid, which resists crystallization.

When, by evaporation, the liquid is reduced to half its quantity, remove it into vessels, six feet high and six inches wide, having a cock at the distance of six inches from the bottom; and there let it remain two or three days. In this interval the juice deposits the rest of its impurities, and especially the gypsum which it retained. At the end of this time draw off the liquid, and pour it again, but only to the height of three inches, into the evaporating copper, and proceed to thicken it by a fire, gradually augmented to ebullition. In proportion as the sugar becomes concentrated, care must be taken to diminish the fire, to prevent it from burning, which would render it quite unfit to be converted into loaf sugar. When the juice has acquired the necessary consistence, the fire must immediately be taken from under the boiler. In half an hour pour the juice, thus boiled to a due consistence, into cones or moulds, the points of which are covered with a piece of linen cloth, and into which has been put a small quantity of sugar-candy broken into coarse pieces; after which remove the moulds into a place whose temperature is between ten and twenty degrees of Reaumur's thermometer. When the different operations have been well executed, the greatest part of the sugar is crystallized in the space of twenty-four hours. If it is boiled too much, the whole is converted into a granulated mass, the interstices of which are filled with melasses.

When all the sugar is well crystallized, uncover the point of the mould, and place it over an earthen vessel, that the melasses may drain off: this, according as the juice is more or less boiled, requires three or four weeks. The sugar remains in the moulds, of a yellow colour, more or less white, and in crystalline grains, of a larger or smaller size, according to the success of the process.

M. Achard, with a view to save time, and to dispense with the necessity of employing vessels for settling, made an alteration in this method, which he at first followed. To the juice, when half evaporated, and gently boiling, he added, for 1,200 pounds of the roots, five quarts of skimmed milk, and a little afterwards one quart of vinegar, and in this manner effected the second clarification immediately in the boiler.

By the process of refining, all the products furnished by West India sugar may be obtained from this sugar of beet-root, and by claying it may be rendered equally white.

V. *An experimental Essay on Salt as a Manure, and as a Condiment mixed with the Food of Animals. By the Rev.* Edmund Cartwright, *of Woburn**.

Were the beneficial effects of salt as a manure to be once fairly ascertained, there can be no doubt but the wisdom of the legislature would devise some means by which, without prejudice to the revenue, the farmer might apply it to the purposes of agriculture.

At present the use of salt as a manure is a subject on which the public opinion is much divided: its advocates, reasoning from the striking effects of salt water on the marshes which are occasionally irrigated by the sea at spring tides, conclude that the fertilizing virtue of such irrigation is owing to its saline quality, without taking into consideration the quantity of animal and vegetable matter which sea water (particularly near the coast, and where rivers disembogue themselves) must necessarily hold in solution.

Those who maintain a contrary opinion, considering salt merely as an antiseptic, satisfy themselves that it is impossible that any thing can be friendly to vegetation which retards putrefaction; a process indispensable in substances that are to be the food of plants. To get over this difficulty, it has been conjectured, nay, there have not been wanting those (and of great name too) who have even attempted to prove, that salt in small quantities accelerates, as in large quantities it is known to resist, putrefaction; a doctrine to which, however, I shall not willingly yield my assent, till I can be persuaded that effects are not, in all cases, proportionate to their causes. The operation of every cause is, and must be, uniform; and when, to appearance, it is not so, some other cause obtrudes itself, too subtile for our observation, which, operating at the same time with the primary cause, joins in giving a result, which not being able to account for, we consider as anomalous.

That theorists should be at variance with each other is not to be wondered at; for, having the wide field of imagination and conjecture before them to expatiate in, it is reasonable to conclude, indeed it is unavoidable, that some of them must lose their way. But what shall we say to the disagreement and inconsistency which prevail on this sub-

* From the *Communications to the Board of Agriculture*, which adjudged to the author the gold medal for this essay.

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ject amongst practical farmers? Nothing, indeed, can be more contradictory than the different reports that have been made on the effects of salt, as a manure, by those who have even brought it to the test of actual experiment. As there is no reason to question the veracity of the reporters, we must look for the grounds of their disagreement in some predominating circumstance or other, which at the time escaped their observation. Indeed, the success or failure of an agricultural experiment depends so frequently on causes which can neither be controlled nor foreseen, and so foreign from those which were expected to operate, that it is not to be wondered at if the repetition of the very same experiment gives oftentimes a different result.

As it is not the business of this paper to support a theory, but to detail what has been practised; not to contend for an opinion, but to state facts; the few observations which may be hazarded will be such only as are required merely in explanation of occurrences as they arise. I shall endeavour to give, therefore, as simple a relation as possible of the experiments I have tried, to ascertain the advantages or disadvantages which may attend the use of salt as a manure, and also when mixed with the food of animals.

It may be necessary, first of all, to premise, that the soil on which my experiments were tried is a ferruginous sand, brought to a due texture and consistence by a liberal covering of pond mud. Of this soil, in its improved state I mean, by the accession of pond mud (for, having been used merely as a nursery for raising forest trees previous to these experiments, the nursery-man had not thought it necessary to make use of any other manure), the following is the analysis:

	Grains.
400 grains gave of siliceous sand of different degrees of fineness about - - - -	280
Of finely divided matter, which appeared in the form of clay - - - - -	104
Loss in water - - - - -	16
	400

The 104 grains of finely divided matter contained of carbonate of lime - - - -	18
Of oxide of iron - - - - -	7
Loss by incineration (most probably from vegetable decomposing matter) - - - -	17

The remainder principally silex and alumine.

 There

There were no indications of either gypsum or phosphate of lime.

It will appear from the above analysis that these experiments could not perhaps have been tried on a soil better adapted to give impartial results; for of its component parts there is no ingredient (the oxide of iron possibly excepted) of sufficient activity to augment or restrain the peculiar energies of the substances employed.

On the 14th of April 1804, a certain portion of this soil was laid out in beds one yard wide and forty long. Of these, twenty-five were manured (the first excepted) as follows:

No. 1. No manure.
2. Salt, $\frac{1}{4}$ peck.
3. Lime, one bushel.
4. Soot, one peck.
5. Wood-ashes, two pecks.
6. Saw-dust, three bushels.
7. Malt-dust, two pecks.
8. Peat, three bushels.
9. Decayed leaves, three bushels.
10. Fresh dung, three bushels.
11. Chandler's graves, nine pounds.
12. Salt, lime.
13. Salt, lime, sulphuric acid.
14. Salt, lime, peat.
15. Salt, lime, dung.
16. Salt, lime, gypsum, peat.
17. Salt, soot.
18. Salt, wood-ashes.
19. Salt, saw-dust.
20. Salt, malt-dust.
21. Salt, peat.
22. Salt, peat, bone-dust.
23. Salt, decayed leaves.
24. Salt, peat-ashes.
25. Salt, chandler's graves.

N. B. The quantities of each ingredient the same as when used singly.

On the same day the whole was planted with potatoes, a single row in each bed; and, that the general experiment might be conducted with all possible accuracy, each bed received the same number of sets.

On the 14th of May, a few days after the plants appeared above ground, the whole was carefully examined, and the comparative excellence of each row (as far at least as could be judged of by appearances) was as carefully registered.

gistered. The best row was decidedly No. 7, malt-dust, after which they followed as under:

No. 11. Chandler's graves.
16. Salt, lime, gypsum, peat.
25. Salt, graves.
20. Salt, malt-dust.
9. Decayed leaves.
4. Soot.
2. Salt.
1. No manure.
5. Wood-ashes.
8. Peat.
13. Salt, lime, sulphuric acid.
14. Salt, lime, peat.
17. Salt, soot.
18. Salt, wood-ashes.
21. Salt, peat.
22. Salt, peat, bone-dust.
23. Salt, decayed leaves.
3. Lime.
6. Saw-dust.
10. Fresh dung.
12. Salt, lime.
15. Salt, lime, dung.
24. Salt, peat-ashes.
19. Salt, saw-dust.

On the 28th of May, fourteen days afterwards, the apparent vigour of the plants was in the following order:

No. 7. Malt-dust.
11. Chandler's graves.
4. Soot.
8. Peat.
16. Salt, lime, gypsum, peat.
17. Salt, soot.
20. Salt, malt-dust.
21. Salt, peat.
23. Salt, decayed leaves.
25. Salt, graves.
1. No manure.
2. Salt.
5. Wood-ashes.
9. Decayed leaves.
13. Salt, lime, sulphuric acid.
14. Salt, lime, peat.
18. Salt, wood-ashes.
24. Salt, peat-ashes.

10. Fresh dung.
3. Lime.
22. Salt, peat, bone-dust.
19. Salt, saw-dust.
15. Salt, lime, dung.
12. Salt, lime.
6. Saw-dust.

On the 21st of September the potatoes were taken up, when the produce of each row was in succession as follows:

No. 17.	Salt and soot produced -	240
11.	Chandler's graves - -	220
18.	Salt, wood-ashes - -	217
16.	Salt, gypsum, peat, lime -	201
15.	Salt, lime, dung - -	199
2.	Salt - - - -	198
25.	Salt, graves - - -	195
4.	Soot - - - -	192
10.	Fresh dung - - -	192
20.	Salt, malt-dust - -	189
5.	Wood-ashes - - -	187
23.	Salt, decayed leaves - -	187
24.	Salt, peat-ashes - - -	185
7.	Malt-dust - - -	184
14.	Salt, lime, peat - -	183
19.	Salt, saw-dust - -	180
22.	Salt, peat, bone-dust - -	178
9.	Decayed leaves - -	175
13.	Salt, lime, sulphuric acid -	175
21.	Salt, peat - - -	171
12.	Salt, lime - - -	167
8.	Peat - - - -	159
1.	No manure - - -	157
6.	Saw-dust - - -	155
3.	Lime - - - -	150

The foregoing table furnishes many particulars worthy of observation. In the first place it is remarkable, that of ten different manures, most of which are of known and acknowledged efficacy, salt, a manure hitherto of an ambiguous character, is superior to them all, one only excepted! And again, when used in combination with other substances, it is only unsuccessfully applied in union with that one, namely, chandler's graves, no other manure seemingly being injured by it. Possibly its deteriorating effects on chandler's graves may be owing to its antiseptic property, which retards the putrefactive process by which animal

animal substances undergo the changes necessary to qualify them to become the food of plants. This, however, I cannot, from any appearance in the soil when the plants were taken up, assert to have been the case.

The extraordinary effects of salt, when combined with soot, are strikingly singular. There is no reason to suppose these effects were produced by any known chemical agency of soot or salt on each other. Were I to guess at the producing cause, I should conjecture it to be that property of saline substances by which they attract moisture from the atmosphere; for I observed those beds where salt had been used were visibly and palpably moister than the rest, even for weeks after the salt had been applied; and this appearance continued till rain fell, when of course the distinction ceased. This property of attracting moisture had greater influence possibly on the soot than on any of the other manures, as soot, from its acrid and dry nature, may be supposed to require a greater proportion of water to dilute it than those substances which contain water already. It may be proper to observe, that on those beds where salt had been used the plants were obviously of a paler green than on the rest, though not less luxuriant; a circumstance which I thought worth noticing, and which I considered, though erroneously (as appeared by the event), to indicate a want of vigour, which would be felt in the crop. It was observable also, that where salt was applied, whether by itself, or in combination, the roots were free from that scabbiness which oftentimes infects potatoes, and from which none of the other beds (and there were in the field nearly forty more than what made part of these experiments) were altogether exempt.

Two sets of experiments, and with the same proportions of manures, were tried with turnips and buck-wheat, on a soil the poorest I could meet with, which produced only a dwarf heath and lichen, and which I had had pared off. The poverty of this soil will appear by the following analysis:

	Grains.
400 grains gave of siliceous sand - -	320
Of finely divided matter, which appeared as brown mould - - - -	68
Loss in water - - - -	12
	400

The finely divided matter lost by incineration nearly half

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its weight; which shows it contained a great deal of vegetable matter. The residuum principally a mixture of aluminous and siliceous earths, coloured red by oxide of iron, and containing very little calcareous matter. There were no indications of either gypsum or phosphate of lime.

July 6, 1804, the pieces set apart for each set of experiments were respectively sown with turnips and buck-wheat.

On the 26th, Nos. 1, 2, 4, 5, 6, 7, 19, 20, 21, 22, 24, 25, showed little or no marks of vegetation. The remainder were merely in the seed-leaf.

On the 16th of August four only were alive, and in rough leaf, namely,

No. 12. Salt and lime.
13. Salt, lime, and sulphuric acid.
14. Salt, lime, peat.
16. Salt, lime, gypsum, peat.

These four maintained a sickly existence till the middle of September; shortly after which they all disappeared.—N. B. The appearances of the turnips and buck-wheat were so nearly uniform, I have not thought it necessary to notice the trifling variations between them, which could not have been done without entering into a minute detail, equally tedious and uninteresting.

Though nothing decisive can probably be drawn from these two sets of experiments respecting the advantages or disadvantages of salt as a manure, on such a soil as I have described, because other manures of acknowledged efficacy shared the same fate with the salt; yet this inference, however, may be drawn from them (and that not an unimportant one), namely, that a due texture and consistence in the soil is as necessary to the existence and health of vegetables, as the pabulum they are sustained by; and this appears evidently by the superiority, such as it was, of those plants where the manure contributed in any degree to improve that texture and consistence.

Adjoining to the place where these experiments were tried is a field, which fully confirms this observation. Within these few years, a great part of it was in a state of uncultivated nature, equally barren as the spot I have been speaking of; it is, however, now brought into cultivation, and into a decent state of fertility, chiefly from its texture having been improved by a thick coating of marly clay.

In trying the effects of salt, when mixed with the food of animals, I have made no experiments on sheep, as I did not apprehend that a few limited experiments would either throw

throw new light upon a subject which has already been sufficiently discussed, as applied to those animals, or furnish the public with facts of which it is not already in possession. My experiments have therefore been confined to hogs and cows.

On July 23d, 1804, three hogs of the same litter, about eight months old, were put up to fatten. Their respective weights were as follow:

No. 1. 44 lbs.
2. 47 lbs.
3. 40 lbs.

From the 23d of July till the 7th of August they were fed with barley-meal mixed up with water; during which time they consumed three bushels and a half of barley, and gained in weight as follows:

No. 1. 12 lbs.
2. 10 lbs.
3. 5 lbs.

From the 3d of August to the 21st, they had salt mixed with their food, of which they consumed one quarter of a pound per day. The food consumed was four bushels: they had gained upon the last weighing as under:

No. 1. 18 lbs.
2. 22 lbs.
3. 14 lbs.

From the 21st of August to the 3d of September the salt was discontinued, in which time they ate four bushels and a half of barley-meal, and their increase of weight was,

No. 1. 24 lbs.
2. 21 lbs.
3. 21 lbs.

From the 3d of September to the 17th they had salt as before, and their consumption of food was the same as during the last fortnight, namely, four bushels and a half of barley-meal. Their gain of weight was,

No. 1. 31 lbs.
2. 19 lbs.
3. 19 lbs.

They were then slaughtered.

It did not appear that the salt had any operation either in promoting thirst or stimulating their appetites, the consumption of food being nearly the same whether salted or not; neither does it appear that the salt had any influence on their fattening; perhaps the quantity allowed them was too little; and yet I should think not, as there was enough

to make their whole mass of food sufficiently savoury to the human taste.

In trying this experiment it will be observed, that I did not confine one parcel of hogs to salt, and another to unsalted food. This mode of trying experiments is always uncertain, as there will be frequently particular habits and tendencies in the individual animals which will vary the esults, and prevent their being uniform. The fairest way, and that which is the least liable to error, is to compare each animal with himself, by feeding him at one period with one kind of food, and then, for an equal period, with another. If this principle which I have proceeded upon be right, there is nothing in these experiments to encourage the practice of administering salt to hogs with a view at least to increase their tendency to fatten; how far it may contribute to keep them in health is a different question, and on which years of experience may probably be necessary to decide. Now I am upon this subject I shall mention (though totally foreign from the object of this essay), that for most internal disorders which hogs are liable to, all of which may be supposed to be more or less accompanied with fever, I find no remedy so efficacious as antimony. This mineral is said to have obtained its name from the head of a religious house, who had administered it with success to his hogs, giving it in such quantities to the monks of his order as to poison them: a circumstance which probably brought it at the time into disrepute as a medicine, as well for the real as the metaphorical hogs. The anecdote, however, whether true or false, induced me some years ago to try it upon hogs; and I can safely aver that, when taken in time, there are few internal diseases which hogs are subject to that will not yield to antimony in some form or other. That form which I prefer is emetic tartar, as lying in small compass. I give it in doses from five to forty or fifty grains, according to the age and strength of my patient; and I believe still larger doses might be given with equal safety, as I do not recollect a single instance in which the animal seemed to suffer from being over-dosed.

To persons who have not tried the effects of antimony on the brute creation, the quantity I give may seem to be strangely disproportionate to the bulk of the hog compared with that of a man; but the experience of many years has convinced me that there is no analogy (I mean as far as quantity is concerned) in the effects of antimony on the human constitution, and on the constitution of inferior animals.

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On the 9th of October 1804, my experiments on cows commenced. On that day two Welch heifers, one of which had calved about five months, the other three, were confined to the house, and fed with hay for the space of one fortnight. The hay they consumed during that time was four hundred weight nineteen pounds, and the milk they produced was thirty-six gallons three quarts. They had then, for the next fortnight, salt mixed with their hay, the hay being first slightly moistened with water, and the salt sprinkled over it; in which time they consumed four hundred weight forty-two pounds of hay, and seven pounds of salt. The milk produced was thirty-seven gallons. For the next fortnight, namely, from the sixth to the twentieth of November, the salt was omitted, and their food was four hundred weight and one quarter of hay, and two hundred weight and a half of cabbages. The produce of milk in that space of time was fifty-four gallons three quarts. From the twentieth of November their food was the same as before, with the addition of half a pound of salt per day. The produce of milk was fifty-seven gallons one quart.

It will be recollected, that salt seemed to have no tendency to promote thirst or to increase appetite in the hogs; yet on the cows its effects in one respect were very perceptible, for during the period they had salt they drank three gallons a day each more than at other times.

Salt may possibly promote digestion, notwithstanding its antiseptic quality, by stimulating the salival glands and the glands yielding the gastric juice, and by inducing an increased discharge of their respective fluids, so necessary to the solubility of the different substances received into the stomach before they can be admitted into the lacteals.

Though there may be nothing in the foregoing experiments to lead us to suppose that salt has any otherwise a tendency to promote a disposition in animals to fatten than as it may contribute to their health by aiding their digestion, yet it is probable that, when administered to animals yielding milk, it may contribute in some small degree to increase that secretion; and this it may do by promoting thirst, which induces the animal to drink copiously; in consequence of which the secretion of milk, as well as all other secretions of the fluids, may be augmented. Perhaps also it may have a stimulating influence on the lacteals themselves.

And yet, after all, admitting these experiments to prove that salt increases in some small degree the production of milk,

milk,—when that increased quantity is balanced against the price of the salt, the dairy-man will find himself no gainer.

Though there does not seem any thing in these experiments, either with hogs or cows, to encourage the practice of giving salt to animals with a view to increase their disposition to fatten, yet it would be temerity to affirm that it is entirely useless. From the avidity with which most if not all kinds of graminivorous animals, whether in a state of domestication or otherwise, are known to eat salt whenever it comes in their way, it is reasonable to conclude that the propensity has not been implanted in them in vain. But from whatever cause its salutary effects may be supposed to proceed, whether (as was hinted at before) from its promoting digestion and an increased secretion of fluids, or from any other action it may have on the animal œconomy, it must be left to an experimenter more successful than I have been, to ascertain.

VI. *On the Analysis of Soils, as connected with their Improvement.* By HUMPHREY DAVY, *Esq. F.R.S. Professor of Chemistry to the Board of Agriculture and to the Royal Institution**.

I. *Utility of Investigation relating to the Analysis of Soils.*

THE methods of improving lands are immediately connected with the knowledge of the chemical nature of soils, and experiments on their composition appear capable of many useful applications.

The importance of this subject has been already felt by some very able cultivators of science; many useful facts and observations with regard to it have been furnished by Mr. Young; it has been examined by Lord Dundonald, in his treatise on the connexion of chemistry with agriculture, and by Mr. Kirwan in his excellent essay on manures: but the inquiry is still far from being exhausted, and new methods of elucidating it are almost continually offered, in consequence of the rapid progress of chemical discovery.

In the following pages I shall have the honour of laying before the Board an account of those methods of analysing soils which appear most precise and simple, and most likely to be useful to the practical farmer; they are founded partly upon the labours of the gentlemen whose names have

* From *Communications to the Board of Agriculture.*

been

been just mentioned, and partly upon some later improvements.

II. *Of the Substances found in Soils.*

The substances which are found in soils, are certain mixtures or combinations of some of the primitive earths, animal and vegetable matter in a decomposing state, certain saline compounds, and the oxide of iron. These bodies always retain water, and exist in very different proportions in different lands; and the end of analytical experiments is the detection of their quantities and mode of union.

The earths found in common soils are principally silex, or the earth of flints, alumine, or the pure matter of clay, lime, or calcareous earth, and magnesia.

Silex, or the earth of flints, when perfectly pure, appears in the form of a white powder, which is incombustible, infusible, insoluble in water, and not acted upon by common acids; it is the substance which constitutes the principal part of rock crystal; it composes a considerable part of hard gravelly soils, of hard sandy soils, and of hard stony lands.

Alumine, or pure clay, in its perfect state is white like silex; it adheres strongly to the tongue, is incombustible, insoluble in water, but soluble in acids, and in fixed alkaline menstrua. It abounds most in clayey soils and clayey loams; but even in the smallest particles of these soils it is usually united to silex and oxide of iron.

Lime is the substance well known in its pure state under the name of quicklime. It always exists in soils in combination, and that principally with fixed air or carbonic acid; when it is called carbonate of lime; a substance which in the most compact form constitutes marble, and in its looser form chalk. Lime, when combined with sulphuric acid (oil of vitriol), produces sulphate of lime (gypsum), and with phosphoric acid, phosphate of lime. The carbonate of lime, mixed with other substances, composes chalky soils and marles, and it is found in soft sandy soils.

Magnesia, when pure, appears as white, and in a lighter powder, than any of the other earths; it is soluble in acid, but not in alkaline menstrua; it is rarely found in soils; when it does exist, it is either in combination with carbonic acid, or with silex and alumine.

Animal decomposing matter exists in very different states, according as the substances from which it is produced are different; it contains much carbonaceous substance,

stance, and may be principally resolved by heat into this substance, volatile alkali, inflammable aëriform products, and carbonic acid; it is principally found in lands that have been lately manured.

Vegetable decomposing matter is likewise very various in kind; it contains usually more carbonaceous substance than animal matter, and differs from it in the results of its decomposition principally in not producing volatile alkali; it forms a great proportion of all peats; it abounds in rich mould, and is found in larger or smaller quantities in all lands.

The saline compounds found in soils are very few, and in quantities so small, that they are rarely to be discovered. They are principally muriate of soda (common salt), sulphate of magnesia (Epsom salt), and muriate and sulphate of potash, nitrate of lime, and the mild alkalies.

The oxide of iron is the same with the rust produced by exposing iron to the air and water; it is found in all soils, but is most abundant in yellow and red clays, and in yellow and red siliceous sands.

A more minute account of these different substances would be incompatible with the object of this paper. A full description of their properties and agencies may be found in the elementary books on chemistry, and particularly in the System of Chemistry by Dr. Thomson (2d ed.); and in Henry's Epitome of Chemistry.

III. *Instruments required for the Analysis of Soils.*

The really important instruments required for the analysis of soils are few, and but little expensive. They are a balance capable of containing a quarter of a pound of common soil, and capable of turning when loaded with a grain; a series of weights from a quarter of a pound troy to a grain; a wire sieve, sufficiently coarse to admit a pepper-corn through its apertures; an Argand lamp and stand; some glass bottles; Hessian crucibles; porcelain or queen's ware evaporating basons; a Wedgewood pestle and mortar; some filters made of half a sheet of blotting-paper, folded so as to contain a pint of liquid, and greased at the edges; a bone knife, and an apparatus for collecting and measuring aëriform fluids.

The chemical substances or reagents required for separating the constituent parts of the soil, are muriatic acid (spirit of salt), sulphuric acid, pure volatile alkali dissolved in water, solution of prussiate of potash, soap lye, solution of carbonate of ammonia, of muriate of ammonia, solution

tion of neutral carbonate of potash, and nitrate of ammonia. An account of the nature of these bodies, and their effects, may be found in the chemical works already noticed; and the reagents are sold, together with the instruments mentioned above, by Mr. Knight, Foster Lane, Cheapside, arranged in an appropriate chest.

IV. *Mode of collecting Soils for Analysis.*

In cases when the general nature of the soil of a field is to be ascertained, specimens of it should be taken from different places, two or three inches below the surface, and examined as to the similarity of their properties. It sometimes happens, that upon plains the whole of the upper stratum of the land is of the same kind, and in this case one analysis will be sufficient; but in valleys, and near the beds of rivers, there are very great differences, and it now and then occurs that one part of a field is calcareous, and another part siliceous; and in this case, and in analogous cases, the portions different from each other should be separately submitted to experiment.

Soils, when collected, if they cannot be immediately examined, should be preserved in phials quite filled with them, and closed with ground glass stoppers.

The quantity of soil most convenient for a perfect analysis is from two to four hundred grains. It should be collected in dry weather, and exposed to the atmosphere till it becomes dry to the touch.

The specific gravity of a soil, or the relation of its weight to that of water, may be ascertained by introducing into a phial, which will contain a known quantity of water, equal volumes of water and of soil; and this may be easily done by pouring in water till it is half full, and then adding the soil till the fluid rises to the mouth; the difference between the weight of the soil and that of the water will give the result. Thus, if the bottle contains four hundred grains of water, and gains two hundred grains when half filled with water and half with soil, the specific gravity of the soil will be two, that is, it will be twice as heavy as water; and if it gained one hundred and sixty-five grains, its specific gravity would be 1825, water being 1000.

It is of importance that the specific gravity of a soil should be known, as it affords an indication of the quantity of animal and vegetable matter it contains; these substances being always most abundant in the lighter soils.

The other physical properties of soils should likewise be examined before the analysis is made, as they denote, to a

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certain extent, their composition, and serve as guides in directing the experiments. Thus, siliceous soils are generally rough to the touch, and scratch glass when rubbed upon it; aluminous soils adhere strongly to the tongue, and emit a strong earthy smell when breathed on; and calcareous soils are soft, and much less adhesive than aluminous soils.

V. *Mode of ascertaining the Quantity of Water of Absorption in Soils.*

Soils, though as dry as they can be made by continued exposure to air, in all cases still contain a considerable quantity of water, which adheres with great obstinacy to the earths and animal and vegetable matter, and can only be driven off from them by a considerable degree of heat. The first process of analysis is, to free the given weight of soil from as much of this water as possible, without, in other respects, affecting its composition; and this may be done by heating it for ten or twelve minutes over an Argand's lamp, in a bason of porcelain, to a temperature equal to 300* Fahrenheit; and in case a thermometer is not used, the proper degree may be easily ascertained, by keeping a piece of wood in contact with the bottom of the dish: as long as the colour of the wood remains unaltered, the heat is not too high; but when the wood begins to be charred, the process must be stopped. A small quantity of water will perhaps remain in the soil even after this operation, but it always affords useful comparative results; and if a higher temperature were employed, the vegetable or animal matter would undergo decomposition, and in consequence the experiment be wholly unsatisfactory.

The loss of weight in the process should be carefully noted; and when in four hundred grains of soil it reaches as high as 50, the soil may be considered as in the greatest degree absorbent, and retentive of water, and will generally be found to contain a large proportion of aluminous earth. When the loss is only from 20 to 10, the land may be considered as only slightly absorbent and retentive, and the siliceous earth as most abundant.

VI. *Of the Separation of Stones, Gravel, and vegetable Fibres from Soils.*

None of the loose stones, gravel, or large vegetable fibres

* In several experiments, in which this process has been carried on by distillation, I have found the water that came over pure, and no sensible quantity of other volatile matter was produced.

should be divided from the pure soil till after the water is drawn off; for these bodies are themselves often highly absorbent and retentive, and in consequence influence the fertility of the land. The next process, however, after that of heating, should be their separation, which may be easily accomplished by the sieve, after the soil has been gently bruised in a mortar. The weights of the vegetable fibres or wood, and of the gravel and stones, should be separately noted down, and the nature of the last ascertained: if calcareous, they will effervesce with acids; if siliceous, they will be sufficiently hard to scratch glass; and if of the common aluminous class of stones, they will be soft, easily scratched with a knife, and incapable of effervescing with acids.

VII. *Separation of the Sand and Clay, or Loam, from each other.*

The greater number of soils, besides gravel and stones, contain larger or smaller proportions of sand of different degrees of fineness; and it is a necessary operation, the next in the process of analysis, to detach them from the parts in a state of more minute division, such as clay, loam, marle, and vegetable and animal matter. This may be effected in a way sufficiently accurate, by agitation of the soil in water. In this case, the coarse sand will generally separate in a minute, and the finer in two or three minutes; whilst the minutely divided earthy, animal, or vegetable matter will remain in a state of mechanical suspension for a much longer time; so that, by pouring the water from the bottom of the vessel, after one, two, or three minutes, the sand will be principally separated from the other substances, which, with the water containing them, must be poured into a filter, and, after the water has passed through, collected, dried and weighed. The sand must likewise be weighed, and their respective quantities noted down. The water of lixiviation must be preserved, as it will be found to contain the saline matter, and the soluble animal or vegetable matters, if any exist in the soil.

VIII. *Examination of the Sand.*

By the process of washing and filtration, the soil is separated into two portions, the most important of which is generally the finely divided matter. A minute analysis of the sand is seldom or never necessary, and its nature may be detected in the same manner as that of the stones or gravel. It is always either siliceous sand, or calcareous sand,

sand, or a mixture of both. If it consist wholly of carbonate of lime, it will be rapidly soluble in muriatic acid, with effervescence; but if it consist partly of this substance, and partly of siliceous matter, the respective quantities may be ascertained by weighing the residuum after the action of the acid, which must be applied till the mixture has acquired a sour taste, and has ceased to effervesce. This residuum is the siliceous part: it must be washed, dried, and heated strongly in a crucible: the difference between the weight of it and the weight of the whole, indicates the proportion of calcareous sand.

IX. *Examination of the finely divided Matter of Soils, and Mode of detecting mild Lime and Magnesia.*

The finely divided matter of the soil is usually very compound in its nature; it sometimes contains all the four primitive earths of soils, as well as animal and vegetable matter; and to ascertain the proportions of these with tolerable accuracy, is the most difficult part of the subject.

The first process to be performed, in this part of the analysis, is the exposure of the fine matter of the soil to the action of the muriatic acid. This substance should be poured upon the earthy matter in an evaporating bason, in a quantity equal to twice the weight of the earthy matter; but diluted with double its volume of water. The mixture should be often stirred, and suffered to remain for an hour or an hour and a half before it is examined.

If any carbonate of lime or of magnesia exist in the soil, they will have been dissolved in this time by the acid, which sometimes takes up likewise a little oxide of iron; but very seldom any alumine.

The fluid should be passed through a filter; the solid matter collected, washed with rain water, dried at a moderate heat, and weighed. Its loss will denote the quantity of solid matter taken up. The washings must be added to the solution; which, if not sour to the taste, must be made so by the addition of fresh acid, when a little solution of common prussiate of potash must be mixed with the whole. If a blue precipitate occurs, it denotes the presence of oxide of iron, and the solution of the prussiate must be dropped in till no further effect is produced. To ascertain its quantity, it must be collected in the same manner as other solid precipitates, and heated red: the result is oxide of iron.

Into the fluid freed from oxide of iron, a solution of neutralized carbonate of potash must be poured till all effervescence

fervescence ceases in it, and till its taste and smell indicate a considerable excess of alkaline salt.

The precipitate that falls down is carbonate of lime; it must be collected on the filter, and dried at a heat below that of redness.

The remaining fluid must be boiled for a quarter of an hour, when the magnesia, if any exist, will be precipitated from it, combined with carbonic acid, and its quantity is to be ascertained in the same manner as that of the carbonate of lime.

If any minute proportion of alumine should, from peculiar circumstances, be dissolved by the acid, it will be found in the precipitate with the carbonate of lime, and it may be separated from it by boiling for a few minutes with soap lye, sufficient to cover the solid matter. This substance dissolves alumine, without acting upon carbonate of lime.

Should the finely divided soil be sufficiently calcareous to effervesce very strongly with acids, a very simple method may be adopted for ascertaining the quantity of carbonate of lime, and one sufficiently accurate in all common cases.

Carbonate of lime, in all its states, contains a determinate proportion of carbonic acid, *i. e.* about 45 per cent.; so that when the quantity of this elastic fluid, given out by any soil during the solution of its calcareous matter in an acid, is known, either in weight or measure, the quantity of carbonate of lime may be easily discovered.

When the process by diminution of weight is employed, two parts of the acid and one part of the matter of the soil must be weighed in two separate bottles, and very slowly mixed together till the effervescence ceases; the difference between their weight before and after the experiment denotes the quantity of carbonic acid lost; for every four grains and a half of which, ten grains of carbonate of lime must be estimated.

The best method of collecting the carbonic acid, so as to discover its volume, is by the pneumatic apparatus, the construction and application of which are described at the end of this paper. The estimation is, for every ounce measure of carbonic acid, two grains of carbonate of lime.

X. *Mode of ascertaining the Quantity of insoluble finely divided Animal and Vegetable Matter.*

After the fine matter of the soil has been acted upon by muriatic acid, the next process is to ascertain the quantity of finely divided insoluble animal and vegetable matter that it contains.

 This

This may be done with sufficient precision, by heating it to strong ignition in a crucible over a common fire till no blackness remains in the mass. It should be often stirred with a metallic wire, so as to expose new surfaces continually to the air; the loss of weight that it undergoes denotes the quantity of the substance that it contains destructible by fire and air.

It is not possible to ascertain whether this substance is wholly animal or vegetable matter, or a mixture of both. When the smell emitted during the incineration is similar to that of burnt feathers, it is a certain indication of some animal matter; and a copious blue flame at the time of ignition almost always denotes a considerable proportion of vegetable matter. In cases when the experiment is needed to be very quickly performed, the destruction of the decomposable substances may be assisted by the agency of nitrate of ammonia, which, at the time of ignition, may be thrown gradually upon the heated mass, in the quantity of twenty grains for every hundred of residual soil. It affords the principle necessary to the combustion of the animal and vegetable matter, which it causes to be converted into elastic fluids; and it is itself at the same time decomposed and lost.

XI. *Mode of separating Aluminous and Siliceous Matter and Oxide of Iron.*

The substances remaining after the decomposition of the vegetable and animal matter, are generally minute particles of earthy matter containing usually alumine and silex with combined oxide of iron.

To separate these from each other, the solid matter should be boiled for two or three hours with sulphuric acid, diluted with four times its weight of water; the quantity of the acid should be regulated by the quantity of solid residuum to be acted on, allowing for every hundred grains two drachms or one hundred and twenty grains of acid.

The substance remaining after the action of the acid may be considered as siliceous; and it must be separated and its weight ascertained, after washing and drying in the usual manner.

The alumine and the oxide of iron, if they exist, are both dissolved by the sulphuric acid; they may be separated by carbonate of ammonia, added to excess; it throws down the alumine, and leaves the oxide of iron in solution; and this substance may be separated from the liquid by boiling.

Should

Should any magnesia and lime have escaped solution in the muriatic acid, they will be found in the sulphuric acid: this, however, is scarcely ever the case; but the process for detecting them, and ascertaining their quantities, is the same in both instances.

The method of analysis by sulphuric acid is sufficiently precise for all usual experiments; but if very great accuracy be an object, dry carbonate of potash must be employed as the agent, and the residuum of the incineration must be heated red for half an hour, with four times its weight of this substance, in a crucible of silver, or of well baked porcelain. The mass obtained must be dissolved in muriatic acid, and the solution evaporated till it is nearly solid; distilled water must then be added, by which the oxide of iron and all the earths, except silex, will be dissolved in combination as muriates. The silex, after the usual process of lixiviation, must be heated red; the other substances may be separated in the same manner as from the muriatic and sulphuric solutions.

This process is the one usually employed by chemical philosophers for the analysis of stones.

XII. *Mode of discovering soluble Animal and Vegetable Matter, and Saline Matter.*

If any saline matter, or soluble vegetable or animal matter, is suspected in the soil, it will be found in the water of lixiviation used for separating the sand.

This water must be evaporated to dryness in an appropriate dish, at a heat below its boiling point.

If the solid matter obtained is of a brown colour and inflammable, it may be considered as partly vegetable extract. If its smell, when exposed to heat, be strong and fœtid, it contains animal mucilaginous or gelatinous substance; if it be white and transparent, it may be considered as principally saline matter. Nitrate of potash (nitre), or nitrate of lime, is indicated in this saline matter, by its scintillating with a burning coal. Sulphate of magnesia may be detected by its bitter taste; and sulphate of potash produces no alteration in solution of carbonate of ammonia, but precipitates solution of muriate of barytes.

XIII. *Mode of detecting Sulphate of Lime (Gypsum) and Phosphate of Lime in Soils.*

Should sulphate or phosphate of lime be suspected in the entire soil, the detection of them requires a particular

process upon it. A given weight of it, for instance four hundred grains, must be heated red for half an hour in a crucible, mixed with one third of powdered charcoal. The mixture must be boiled for a quarter of an hour, in a half-pint of water, and the fluid collected through the filter, and exposed for some days to the atmosphere in an open vessel. If any soluble quantity of sulphate of lime (gypsum) existed in the soil, a white precipitate will gradually form in the fluid, and the weight of it will indicate the proportion.

Phosphate of lime, if any exist, may be separated from the soil after the process for gypsum. Muriatic acid must be digested upon the soil, in quantity more than sufficient to saturate the soluble earths; the solution must be evaporated, and water poured upon the solid matter. This fluid will dissolve the compounds of earths with the muriatic acid, and leave the phosphate of lime untouched.

It would not fall within the limits assigned to this paper, to detail any processes for the detection of substances which may be accidentally mixed with the matters of soils. Manganese is now and then found in them, and compounds of the barytic earth; but these bodies appear to bear little relation to fertility or barrenness, and the search for them would make the analysis much more complicated, without rendering it more useful.

XIV. *Statement of Results and Products.*

When the examination of a soil is completed, the products should be classed, and their quantities added together; and if they nearly equal the original quantity of soil, the analysis may be considered as accurate. It must however be noticed, that when phosphate or sulphate of lime are discovered by the independent process XIII., a correction must be made for the general process, by subtracting a sum equal to their weight from the quantity of carbonate of lime obtained by precipitation from the muriatic acid.

In arranging the products, the form should be in the order of the experiments by which they were obtained.

Thus, 400 grains of a good siliceous sandy soil may be supposed to contain

	Grains.
Of water of absorption - - -	18
Of loose stones and gravel, principally siliceous,	42
Of undecompounded vegetable fibres -	10
Of fine siliceous sand - - -	200
	270

Brought

		Grains.
Brought over -		270
Of minutely divided matter separated by filtration, and consisting of		
Carbonate of lime - -	25	
Carbonate of magnesia - -	4	
Matter destructible by heat, principally vegetable, - - -	10	
Silex - - - -	40	
Alumine - - - -	32	
Oxide of iron - - -	4	
Soluble matter, principally sulphate of potash and vegetable extract, -	5	
Gypsum - - - -	3	
Phosphate of lime - -	2	
		125
Amount of all the products		395
Loss - -		5

In this instance the loss is supposed small; but in general, in actual experiments, it will be found much greater, in consequence of the difficulty of collecting the whole quantities of the different precipitates; and when it is within thirty for four hundred grains, there is no reason to suspect any want of due precision in the processes.

XV. *This general Method of Analysis may in many Cases be much simplified.*

When the experimenter is become acquainted with the use of the different instruments, the properties of the reagents, and the relations between the external and chemical qualities of soils, he will seldom find it necessary to perform, in any one case, all the processes that have been described. When his soil, for instance, contains no notable proportion of calcareous matter, the action of the muriatic acid IX. may be omitted. In examining peat soils, he will principally have to attend to the operation by fire and air X.; and in the analysis of chalks and loams, he will often be able to omit the experiment by sulphuric acid XI.

In the first trials that are made by persons unacquainted with chemistry, they must not expect much precision of result. Many difficulties will be met with; but, in overcoming them, the most useful kind of practical knowledge will be obtained; and nothing is so instructive in experimental science as the detection of mistakes. The correct analyst

analyst ought to be well grounded in chemical information; but perhaps there is no better mode of gaining it, than that of attempting original investigations. In pursuing his experiments, he will be continually obliged to learn from books the history of the substances he is employing or acting upon; and his theoretical ideas will be more valuable in being connected with practical operation, and acquired for the purpose of discovery.

XVI. *On the Improvement of Soils, as connected with the Principle of their Composition.*

In cases when a barren soil is examined with a view to its improvement, it ought in all cases, if possible, to be compared with an extremely fertile soil in the same neighbourhood, and in a similar situation: the difference given by their analyses would indicate the methods of cultivation; and thus the plan of improvement would be founded upon accurate scientific principles.

If the fertile soil contained a large quantity of sand, in proportion to the barren soil, the process of amelioration would depend simply upon a supply of this substance; and the method would be equally simple with regard to soils deficient in clay or calcareous matter.

In the application of clay, sand, loam, marle, or chalk, to lands, there are no particular chemical principles to be observed; but when quicklime is used, great care must be taken that it is not obtained from the magnesian limestone; for in this case, as has been shown by Mr. Tennant, it is exceedingly injurious to land*. The magnesian limestone may be distinguished from the common limestone by its greater hardness, and by the length of time that it requires for its solution in acids, and it may be analysed by the process for carbonate of lime and magnesia IX.

When the analytical comparison indicates an excess of vegetable matter, as the cause of sterility, it may be destroyed by much pulverization and exposure to air, by paring and burning, or the agency of lately made quicklime. And the defect of animal and vegetable matter must be supplied by animal or vegetable manure.

XVII. *Sterile Soils in different Climates and Situations must differ in Composition.*

The general indications of fertility and barrenness, as found by chemical experiments, must necessarily differ in

* Phil. Transactions for 1799, p. 305. This limestone is found abundantly in Yorkshire, Derbyshire, and Somersetshire.

different climates, and under different circumstances. The power of soils to absorb moisture, a principle essential to their productiveness, ought to be much greater in warm and dry countries than in cold and moist ones; and the quantity of fine aluminous earth they contain larger. Soils likewise that are situated on declivities ought to be more absorbent than those in the same climate on plains or in valleys*. The productiveness of soils must likewise be influenced by the nature of the subsoil, or the earthy or stony strata on which they rest; and this circumstance ought to be particularly attended to, in considering their chemical nature, and the system of improvement. Thus, a sandy soil may sometimes owe its fertility to the power of the subsoil to retain water; and an absorbent clayey soil may occasionally be prevented from being barren, in a moist climate, by the influence of a substratum of sand or gravel.

XVIII. *Of the chemical Composition of fertile Corn Soils in the Climate.*

Those soils that are most productive of corn contain always certain proportions of aluminous and calcareous earth in a finely divided state, and a certain quantity of vegetable or animal matter.

The quantity of calcareous earth is however very various, and in some cases exceedingly small. A very fertile corn soil from Ormiston in East Lothian afforded me in a hundred parts, only eleven parts of mild calcareous earth; it contained twenty-five parts of siliceous sand; the finely divided clay amounted to forty-five parts. It lost nine in decomposed animal and vegetable matter, and four in water, and afforded indications of a small quantity of phosphate of lime.

This soil was of a very fine texture, and contained very few stones or vegetable fibres. It is not unlikely that its fertility was in some measure connected with the phosphate; for this substance is found in wheat, oats, and barley, and may be a part of their food.

A soil from the low lands of Somersetshire, celebrated for producing excellent crops of wheat and beans without manure, I found to consist of one-ninth of sand, chiefly siliceous, and eight-ninths of calcareous marle tinged with iron, and containing about five parts in the hundred of vegetable matter. I could not detect in it any phosphate or sulphate of lime; so that its fertility must have depended

* Kirwan. Trans. Irish Academy, vol. v. p. 175.

principally upon its power of attracting principles of vegetable nourishment from water and the atmosphere*.

Mr. Tillet, in some experiments made on the composition of soils at Paris, found that a soil composed of three-eighths of clay, two-eighths of river sand, and three-eighths of the parings of limestone, was very proper for wheat.

XIX. *Of the Composition of Soils proper for bulbous Roots and for Trees.*

In general, bulbous roots require a soil much more sandy and less absorbent than the grasses. A very good potatoe soil, from Varsel in Cornwall, afforded me seven-eighths of siliceous sand; and its absorbent power was so small, that one hundred parts lost only two by drying at 400 Fahrenheit.

Plants and trees, the roots of which are fibrous and hard, and capable of penetrating deep into the earth, will vegetate to advantage in almost all common soils which are moderately dry, and which do not contain a very great excess of vegetable matter.

I found the soil taken from a field at Sheffield-place in Sussex, remarkable for producing flourishing oaks, to consist of six parts of sand, and one part of clay and finely divided matter. And one hundred parts of the entire soil, submitted to analysis, produced

	Parts.
Water - - - - - -	3
Silex - - - - -	54
Alumine - - - - -	28
Carbonate of lime - - -	3
Oxide of iron - - - -	5
Decomposing vegetable matter -	4
Loss - - - - -	3

XX. *Advantages of Improvements made by changing the Composition of the earthy Parts of Soils.*

From the great difference of the causes that influence the productiveness of lands, it is obvious that, in the present state of science, no certain system can be devised from their improvement, independent of experiment: but there are few cases in which the labour of analytical trials will not be amply repaid by the certainty with which they denote the best methods of amelioration; and this will particularly

* This soil was sent to me by T. Poole, Esq. of Nether Stowey. It is near the opening of the river Parret into the British Channel; but, I am told, is never overflowed.

happens

happen when the defect of composition is found in the proportions of the primitive earths.

In supplying animal or vegetable manure, a temporary food only is provided for plants, which is in all cases exhausted by means of a certain number of crops; but when a soil is rendered of the best possible constitution and texture, with regard to its earthy parts, its fertility may be considered as permanently established. It becomes capable of attracting a very large portion of vegetable nourishment from the atmosphere, and of producing its crops with comparatively little labour and expense.

Description of the Apparatus for the Analysis of Soils. See Plates II. and III.

A. Retort.

B. B. Funnels for the purpose of filtrating.

D. Balance.

E. Argand's lamp.

F, G, H, K. The different parts of the apparatus required for measuring the quantity of elastic fluid given out during the action of an acid on calcareous soils. F represents the bottle for containing the soil. K. The bottle containing the acid furnished with a stopcock. G. The tube connected with a flaccid bladder. I. The graduated measure. H. The bottle for containing the bladder. When this instrument is used, a given quantity of soil is introduced into F; K is filled with muriatic acid diluted with an equal quantity of water; and the stopcock being closed is connected with the upper orifice of F, which is ground to receive it. The tube G is introduced into the lower orifice of F, and the bladder connected with it placed in its flaccid state into H, which is filled with water. The graduated measure is placed under the tube of H. When the stopcock of K is turned, the acid flows into F, and acts upon the soil; the elastic fluid generated passes through G into the bladder, and displaces a quantity of water in H equal to it in bulk, and this water flows through the tube into the graduated measure; the water in which gives by its volume the indication of the proportion of carbonic acid disengaged from the soil; for every ounce measure of which two grains of carbonate of lime may be estimated.

L. Represents the stand for the lamp.

M, N, O, P, Q, R, S. Represent the bottles containing the different reagents.

VII. *Ex-*

VII. *Extract from a Memoir on the Steeping of Wool, and on the Influence of its different States on Dyeing. By M. J. L. Roard, Director of the Dyeing Establishment in the Imperial Manufactories. Read in the French National Institute*.*

Notwithstanding the labours of Dufay, of Hellot, of Macquer, and the important investigations of Messrs. Chaptal and Berthollet, dyeing still presents a great number of problems, which are difficult to be resolved, owing to the number and variety of its agents. Besides the effects produced by the nature of the primary substances, by the action of water, of air, of caloric, and by the degree of attraction of the colouring principles for vegetable and animal substances, differences which exist in the state of the substances to be dyed occasion very remarkable alterations. M. Roard, who is charged with the superintendance of the dye-houses belonging to the imperial manufactories, has constantly observed that wools of various qualities, subjected to the same experiments, were coloured in a manner more or less intense, whenever he was desirous of forming a comparison between them. These differences in the degree of affinity for the colouring particles are owing to a modification of the wool, of which he intends to treat in another memoir.

The effects which particularly excited his astonishment were those presented by wools perfectly alike in their external qualities, which assumed in the same vat very different colours. It was of the greater importance to inquire into the cause of this difference, as dyeing, whose influence over many of the arts is so powerful, is the basis of the manufactories of tapestry; and as the slightest error in the production of a colour renders it totally useless and unserviceable. This strictness in the choice of colours is in an especial manner observed in the manufactory of the Gobelins. The zeal and the exertions of M. Guillaumot, and his indefatigable perseverance in destroying deep-rooted prejudices, have brought that establishment to such a degree of splendour and perfection, that the pictures of the most celebrated painters are transferred to its productions in a manner equally accurate and astonishing. The execution of the tints destined for that manufacture is at present attended with the greater difficulties, because, instead of

* From the *Annales de Chimie*, No. 153.

operating,

operating, as formerly, with a series of colours taken as it were at random, it is necessary to find precisely the shade required, to follow the insensible gradation from light to dark through an harmonious succession of thirty or forty colours. But how can a dyer, however he may be accustomed to all the operations of this kind, be sure of obtaining invariable results, when, besides a multitude of well known causes, minute differences in the degree of twisting alter the affinity of the light, and when the least mixture in the substances to be dyed causes a considerable variation in the affinity for the colouring principle? Previous experiments made on the wool of animals in different states, caused M. Roard to imagine that a more extensive investigation of the subject would make him acquainted with the cause of the changes he had before observed.

M. Tessier, to whom agriculture owes such important improvements, facilitated his researches by procuring him fleeces of Merinos in the grease from animals in health, diseased, and such as had died of the rot.

The wools of the healthy, dead, and diseased animals, corresponding to the numbers 1, 2, 3, were employed separately, together, and mixed with scrapings (*pelure*), wool of very inferior quality, and which has besides been altered by lime.

Scouring and bleaching are so intimately connected with the operations of dyeing, that the author thought fit to begin his comparative observations with these preliminary processes, and even to extend them to the grease, the constituent principles of which were precisely explained in M. Vauquelin's memoir on the nature of that substance.

The agents which he employed for scouring wool, either in the fleece or spun, are: 1. Grease; 2. Soap; 3. Caustic potash; 4. Hot water; 5. Boiling water; 6. Flanders soap.

1. These wools being treated separately, according to the universal custom, were not completely freed from grease. No. 1. was very white, perfectly free from all impurity, without the smell of sheep; but, on rubbing it between the fingers, a matter somewhat greasy might be perceived. The wool of the beast No. 2, which had died of the rot, was extremely dirty, charged with earth and animal matters: after being scoured it had still a yellowish gray colour, some smell, and was more greasy than the preceding. In the fleece of No. 3, attacked with a languid disease, were a great quantity of ticks. That insect had not a little contributed to aggravate the disease of the animal, whose soft, weak

weak wool was of a greenish yellow colour, which distinctly announced its decay.

2. A portion of the wool of each of the preceding numbers, being treated hot with 1-20th of soap, became very white, and perfectly free from the grease: it had a very little smell, which exposure to the air speedily removed.

3. One-fortieth part of caustic potash scours and whitens wool extremely well; but this method, though very efficacious, is attended with too many inconveniences to advise its employment.

4. Wool steeped for some time in hot water lost, by the action of the potash, too little of its greasy matter to be employed in that state.

5. It is dangerous in all the operations of scouring to raise the temperature of the fluid above 60°, or to leave the wool in it longer than a quarter of an hour, for it is liable to be very soon injured in boiling water.

6. Flanders soap is the substance which appeared to act in the most advantageous manner; it scours very speedily, and gives wool a degree of whiteness which it is extremely difficult to produce by any other means.

On comparing wool spun in the grease and afterwards scoured, with that scoured before it was spun, it appeared that the former had become exceedingly white, resembling the colour of unwrought cotton, while the second retained a dull yellow cast, from which it can never be freed. This last experiment frequently repeated, and in several different ways, constantly afforded the same results. It perfectly agrees with the ideas current in the work-shops, that wool badly scoured can never be thoroughly cleansed from grease, and that a great part of the preparations it may receive in dyeing are never fixed in a solid manner. Thus, besides the advantage of sparing proprietors an operation which they never execute perfectly, a twofold cause ought to induce them to preserve wool in the grease: in the first place, to protect it from insects and grubs, which seldom attack it in that state; and in the second, to allow the various arts which employ white wool the means of giving it that purity and lustre which it can never acquire when it has been previously scoured.

The effects of gas and sulphuric acid were likewise tried; but neither of those means was capable of giving to wool, twice scoured, the same degree of whiteness as to that which had been completely freed from grease at once.

These experiments served to ascertain a fact which he had

had long before observed; namely, that whiteness, so far from being the same in substances belonging to different classes, varies even in the produce of the same class: thus, the white of cotton will never be the same as that of thread, and a difference will always be perceived between the whiteness of wool and that of silk, in the same manner as we distinguish, though with greater difficulty, the numerous products of individuals of the same family. For it cannot be doubted that, if even the same disposition of the surfaces could establish between all these bodies a certain identity for reflecting the light; yet, as the smallest difference of their nature must affect their affinity, this alone would be sufficient to produce alterations in them.

The intention of these researches was to ascertain the influence which the state of the animal must exercise over the grease, and the nature of the wool.

The grease is a fatty, unctuous substance, with a very strong smell, which is supplied in the sheep by sweat, and the transpirable matter emitted by all animals. When dissolved in water, and filtered to disengage it from the earthy and animal matters which adhere to it, it is of a beautiful yellow fawn colour, more or less inclined to red, and composed, according to M. Vauquelin, of a soap with a basis of potash, animal matter, lime, and potash, combined with carbonic, acetic, and muriatic acids. Filtration likewise separates a white matter floating on the surface of the grease, and which in scouring does not combine with the alkalies: it appears to be of the same nature as suet; it melts, and becomes liquid, at a low temperature, and takes fire very easily.

The animal matter dissolved by alkalies is precipitated of a reddish yellow by all the acids. Oxygenated muriatic acid and oxygenated muriatic acid gas form in it a white flaky precipitate, which becomes coloured by exposure to the air: it is a kind of paste, soft, somewhat viscous, of a dirty yellow colour; it speedily becomes liquid, and burns with a bright white flame. This matter, when kept for some hours at eighty degrees, in several pounds of water, is totally insoluble; but by evaporating the liquid you obtain a small quantity of a soft matter, of a dark brown colour, which has an agreeable smell, resembling that of the extract of liquorice. He was the less surprised to find this smell in the grease, as in his experiments made in the year 1800, by which he first demonstrated the presence of potash in it, he remarked that ammonia, kept in digestion with this substance, gave it a strong smell of orange flowers,

and

and that all the antient writers who treat of the medical properties of the *œsype*, and its fœtid smell, agree that after a very long period it changes to an agreeable odour, resembling that of ambergris.

Alcohol treated with the animal matter takes up a resinous substance of a pale yellow colour, which is precipitated in white flakes, of a light yellow, by water and by acids.

Being desirous of attacking the yellow extremities of the parts of the wool from under the belly and the thighs of the animal, he treated them with alcohol, quicklime, and caustic alkalies; but none of these agents was capable of changing their colour. It appears that the grease accumulated in those parts, together with the action of the air, produces a very intimate combination, which cannot be destroyed without injuring their texture.

Equal portions of the grease, Nos. 1, 2, 3, at the same degree of concentration, were filtered and evaporated nearly to dryness in capsules of porcelain. No. 1. furnished twice as much matter as the two others; it strongly attracted the humidity of the air, and became almost entirely liquefied by it. Acids acted on all three in the same manner, producing a very decisive effervescence. After burning them in a crucible of platina, he separated from all three, by distilled water or by nitric acid, potash slightly carbonated, or nitrate of potash, the weight of which was more considerable in No. 1. than in 2 and 3, which exhibited no very perceptible difference.

These experiments, by demonstrating that the grease and the potash, one of its component principles, increase or decrease in the Merinos, according to their state of health or of disease, enable us likewise to form a judgment of the immediate relation of this substance to these different states, as also of its influence on the beauty of their products. For it would be a great mistake to look upon it as prejudicial to them, when we know that the augmentation of this secretion is incapable of altering the health of the animals, as remarked by Messrs. Gillert, Tessier, and Huzard, in their observations on the growth of long wools; and when the most celebrated naturalists agree in rejecting every method tending to deprive them of it, such as exposing them to long rains, and washing their backs. Besides, does not the Merino, which is the most distinguished of all the species of this genus for the fineness and the beauty of its rich fleece, yield the greatest quantity of grease? and do we not see this substance diminishing with the quality of the wool,

and

and dwindling to nothing in those of the same species that are covered with hair, as the sheep of Guinea and Senegal?

As M. Roard cannot at present give his observations all the latitude of which they are susceptible, he hastens to make public the experiments relative to the effects produced in dyeing by the different qualities of the wools he employed.

The colour assumed by wools while steeping appeared to him a fact so interesting, that he thought it necessary to investigate the cause. He alternately changed the vessels and the agents destined for this operation, and ascertained that this coloration ought to be ascribed entirely to the action of copper; for ammonia forms a blue precipitate in steeping vessels of that metal, while the same precipitate is extremely white, if vessels of earth, porcelain, or even tin, be employed. Wool left for some hours in boiling water, in a copper vessel, acquires a greenish gray tint; but this effect is greatly augmented by the ordinary mixture of alum and tartar. If into this bath, saturated and boiling, you plunge different kinds of combed wool, those produced by the native breed of France and Holland assume a lively green colour, and those of Merinos a greenish yellow, or a very dark ochre yellow. Though this effect is much less perceptible in steeps on a large scale, yet, by comparing white wool with that which has been steeped, the difference appears sufficiently striking. The colour fixed by this method is very little altered by alkalies, and not at all by acids, which in a slight degree heighten its intensity: ammonia turns it to a yellowish gray.

In these experiments the author employed alum manufactured by M. Curaudau, which appeared to him to possess all the qualities and defects of Roman alum, in a comparative investigation which he undertook relative to the effects in dyeing of all the kinds sold in the shops.

The wools, after remaining eight days in the alum liquor, were then dyed with cochineal, madder, saunders wood, &c. The same qualities, whether natural or acquired, having appeared to act in the same manner in all the experiments to which they were subjected, M. Roard describes only the first, which was that made with cochineal.

Experiment I.

No. 1. *Healthy Merinos.*

A beautiful carnation red, inclining a little to yellow. This No. 1. surpassed in depth and intensity all the shades which he tried of more than two or three colours.

Experiment

Experiment II.

Nos. 2 and 3. *Animals dead and diseased.*

The colours almost always the same; sometimes, however, No. 3 is less highly coloured. The difference between these wools of dead and diseased animals and those of healthy sheep, though both of the same flock, is very remarkable.

Experiment III.

No. 4. *A Mixture of equal Parts of* 1, 2, *and* 3.

The quantity of the altered wool being much greater in this experiment, the colours I obtained with it nearly resemble those of 2 and 3, but never equal in beauty that of No. 1.

Experiment IV.

No. 5. *The same Wool as No.* 1, *but spun without Oil, and cleared of the Grease at a single Operation.*

The colour produced by this wool was more brilliant than that of No. 1, but its tone was less high; which demonstrates that in some operations the natural colouring matter must be of some utility. Thus, in fine crimsons, and some other colours, the silks ought to retain somewhat of their rawness, for those that are white can never acquire the same appearance. This observation perfectly coincides with the experiments of Coulomb on the good effects in dyeing of silk still charged with a portion of its colouring principle.

Experiment V.

No. 6. *Clippings of Wool of Picardy.*

The deteriorated matter, which forms a part of this wool, takes the colour so ill, that it is always clouded: in all the experiments it invariably produced a dull dirty colour, far inferior to that of No. 1. By the mixture of this damaged wool, the dealers adulterate the quality of all the carded wools of France, which, as much preparation agrees very ill with them, can be employed only in the manufacture of the most ordinary stuffs.

Experiment VI.

No. 7. *Clippings and Wool of the Merinos No.* 1, *in equal Parts.*

Notwithstanding the bad quality of the wool No. 6, this mixture took the colour so well, that, in all my experiments, it was not much inferior to that of No. 1, though, owing to the clippings, its appearance was always dull.

Experiment

Experiment VII.

Nos. 8 and 9. *Mixtures made with equal Parts of Clippings, the Wool of dead Animals No. 2, and of diseased Sheep No. 3.*

The difference between Nos. 8 and 9 was scarcely perceptible; the colours were dull and dirty, and darker than Nos. 2 and 3, of which they were in part formed.

Experiment VIII.

The same numbers of wool employed in the preceding experiments were dyed blue, and their results perfectly agreed with those already stated. This colour is perhaps the only one that wools of an inferior quality take well, though the blue is not equal, and always inclines to black.

Experiment IX.

The wools Nos. 1 and 2, which had been scoured, and the scrapings, No. 8, were treated with the dye, comparatively with that of No. 1, spun in the grease. The three first took the colour slowly, and assumed a dull blue tint, inclining to black. No. 1, on the contrary, took it very speedily, and acquired a beautiful and very deep blue colour. These four numbers were scoured together, hot, with Flanders soap: the wools of the healthy and dead animals, and the clippings, entirely lost their colour; while that of No. 1, in the grease, retained a very brilliant barbel blue.

Experiment X.

Wools of the three qualities employed in the three manufactories of tapestry were dyed at the same time with the Merino wool No. 1. In all the experiments the latter took a deeper colour than any of the others, which are carded wools of Flanders, Holland, and Picardy.

The principal facts contained in this memoir lead to the following consequences:

1. In scouring, the heat of the fluid ought never to exceed 60°; for, even before it rises to the temperature of boiling water, wools in the grease are very liable to be injured by the potash.

2. Wools scoured at two operations can never be rendered completely white. This effect seems to proceed from a change of state in the greasy colouring matter, which, by becoming more highly oxygenated, loses its solubility.

3. Oxygenated muriatic acid, and oxygenated muriatic acid gas, precipitate, in white flakes, the animal matter contained in the grease: it is speedily coloured by the air, and

and contains a substance with an agreeable smell, which appears to be perfectly analogous to that developed by ammonia, and with that discovered in it by the antients.

4. It ought to excite little surprise to see the quantity of potash and of grease diminish or increase in sheep according to their state of disease or of health; for a secretion so complicated, requiring the utmost exertions of nature, must invariably be intimately connected with the augmentation or diminution of the vital powers. But how is it possible to doubt that the grease has an immediate action on the quality of the wool, when we see those two substances proceed, if we may so express it, in harmony, from the wild sheep of Greece to the most beautiful and the most vigorous Merinos? It was probably to assist them to recover this precious transpiration, that the Romans, after shearing, covered them with a mixture of tonic and oily substances, which, according to Columella, preserved them from many diseases, and contributed to render their wools finer and longer.

5. These wools constantly assume, in copper vessels, solid colours, more or less deep, which, even at the lowest degree of coloration, prevent them from taking the first shades of a tint. This effect is obviated by the use of tin vessels, the oxide of which cannot alter the whiteness of the wool during steeping.

6. All the experiments prove that the affinity for the colouring matter varies in wools according to the healthy or diseased state of the animal; and that the wool of healthy Merinos is always more highly coloured than not only Nos. 2 and 3, though the produce of the same flock, but even than all the carded wools of France and Holland. They show to what causes we ought to ascribe the effects produced on wools the exterior characters of which are perfectly alike, and which, after receiving the same preparations, assume, in the same vat, different colours.

7. The beautiful and very solid blue colours obtained from wools in the grease, demonstrate, in a very positive manner, the influence of that animal matter, which, if transferred to other substances, might furnish the arts with many highly useful applications.

Observations of the Author.

Since the reading of my memoir to the National Institute, I have received a complete proof of the facts to which I ascribe the variations exhibited in dyeing by carded wools. Having ascertained that the different causes which exercised

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an influence over our operations could not arise from the manipulations of the dyer, we complained to our wool-merchant of the bad quality of his goods. He was then obliged to acknowledge that he mixed the wools of Flanders with those of Holland according to the general practice of the trade; and that, though all the dyers had constantly complained of the same defects, yet, as they had neglected to acquaint him with the cause, he had not been able to take such measures as to prevent them in future. These wools are likewise attended with a disadvantage of another kind, which it is of considerable importance to indicate; I mean the augmentation that is given them by passing them through butter-milk, and which almost always amounts to one-eighth of their weight. They are surcharged with a white dusty matter; which, even after careful and repeated washing, still furnishes a sufficient quantity of acetous acid to change a great number of results in dyeing.

VIII. *New Galvanic Discoveries.* *By* M. RITTER. *Extracted from a Letter from* M. CHRIST. BERNOULLI*.

1. *Charging a Louis d'Or by the Pile.*

THE pile with which M. Ritter commonly performs his experiments consists of 100 pairs of plates of metal, two inches in diameter; the pieces of zinc have a rim to prevent the liquid pressed out from flowing away; and the apparatus is insulated by several plates of glass.

As M. Ritter resides at present near Jene, I have not had an opportunity of seeing experiments with his great battery of two thousand pieces, or with his battery of fifty pieces, each thirty-six inches square, the action of which continues very perceptible for a fortnight. Neither have I seen his experiments with the new battery of his invention, consisting of a single metal, and which he calls *the charging pile.*

I have, however, seen him galvanize a louis d'or. He places it between two pieces of pasteboard, thoroughly wetted, and keeps it six or eight minutes in the chain of circulation connected with the pile; and thus the louis becomes charged, though not immediately in contact with the conducting wires. If the louis thus charged be applied to the crural nerves of a frog recently prepared, the usual con-

* Abridged from *Van Mons's Journal,* vol. vi.

tractions are excited. I had put a louis thus galvanised into my pocket, and M. Ritter said to me a few minutes after, that I might find out this louis from among the rest, by trying them in succession upon the frog. Accordingly I made the trial, and actually distinguished, among several others, a single one, in which the exciting quality was very evident. This charge is retained in proportion to the time that the piece has remained in the circuit of the pile. Of three different louis which M. Ritter charged in my presence, neither lost its charge in less than five minutes. These experiments succeeded completely, and nothing seemed so easy as to repeat them.

A metal thus retaining the galvanic charge, though in contact with the hand and with other metals, shows that this communication of the galvanic virtue has more affinity with magnetism than with electricity, and assigns io the galvanic fluid an intermediate rank between these two.

M. Ritter can, in the way I have just described, charge at once any number of pieces. It it only necessary that the two extreme pieces of the number communicate with the pile through the intervention of wet pasteboards. It is with metallic discs charged in this manner, and placed upon one another with pieces of wet pasteboard alternately interposed, that M. Ritter constructs his charging pile, which ought in remembrance of its inventor to be called the *Ritterian pile.* The construction of this pile shows, that each metal galvanised in this way acquires polarity, as the needle does when touched with a magnet. Though I have had no opportunity of seeing this new pile, I have convinced myself of the reality of the phænomenon by an experiment of the highest importance to science, and for the invention of which we are equally indebted to the same ingenious philosopher.

M. Ritter, in his numerous experiments on the excitation produced in the frog by the contact of two different metals, (for he has not entirely abandoned the original mode of galvanising, like most other experimentalists, who employ Volta's pile exclusively,) had perceived not only a very striking difference in the excitability of the different parts of animals, but also a difference of excitement between the extensor and flexor muscles, according as the positive or negative pole was applied to them, or as they were acted upon the instant after the metals were brought into contact or separated from each other.

When the excitability is at its highest point of energy, as in very young frogs the moment after they are prepared, or in

in adult frogs during the coupling season, the flexors alone contract, and in particular the flexor muscles of that thigh to which the silver or negative metal is applied, contract at the instant when the metals come into contact, while those of the thigh to which the zinc or positive metal is applied, contract at the instant of their separation.—Opposite effects are observable in frogs the excitability of which is on the point of being extinguished (Ritter's fifth degree). In this case the extensors only contract, and the flexors remain absolutely motionless. At the moment of contact of the metals the muscles on the zinc side alone are thrown into action, and at the moment of separation those on the silver side.

M. Ritter distinguishes three degrees of mean excitability. At the second degree (the first of the three mean degrees), when the metals are brought into contact, a strong excitement of the flexors takes place on the silver side, and a weak excitement of the extensors on the zinc side; and when the metals are separated a strong excitement of the flexors is seen on the zinc side, and a weak excitement of the extensors on the silver side.

At the fourth degree of excitability the contrary takes place. At the third or middle degree the excitability appears to be equally distributed, the contractions on each side appearing equal, and at the moment of contact the flexors contract on the silver side, the extensors on the zinc side; while at the moment of separation the extensors contract on the silver side, and the flexors on the zinc side.

All these phænomena were exhibited to me by M. Ritter, and the different contractions were very easy to be distinguished. I have not had time to repeat these experiments, and I fear, easy as they appeared to be, they will require an experienced hand to produce such distinct effects as I witnessed. Even with him, none of the experiments which I saw succeeded the first time.

M. Ritter, after showing me his experiments on the different contractibility of various muscles, made me observe, that the piece of gold galvanised by communication exerts at once the action of two metals, or of one constituent part of the pile; and that the half which in the circle was next the negative pole became positive, and the half towards the positive pole became negative.

Having discovered a way to galvanise metals, as iron is rendered magnetic, and having found that the galvanised metals always exhibit two poles, as the magnetic needle does, M. Ritter suspended a galvanised gold needle on a pivot. He perceived, to his surprise, that these needles

had a certain dip and variation, and that the angle of variation, the quantity of which I am sorry I cannot recollect, was uniformly the same in all his experiments. It differs however from that of the magnetic needle, and the positive pole always dips.

IX. *Galvanic Experiments. By M.* Ritter*.

Though I have for some time past employed myself more in the physical than in the chemical part of galvanism, I have, however, had occasion to make some observations in the latter department. I have, for example, formed of the oxide of iron an indigo blue extremely beautiful. To obtain this oxide you must take a glass tube six inches long, with a diameter of half or three quarters of an inch. You fill the tube a third or fourth part with mercury, and the rest with water. Introduce into each end of the tube a thick iron wire; place the tube in a somewhat inclined position, and make the superior wire communicate with the positive pole, and the inferior wire with the negative pole. After some hours the surface of the mercury will be found covered with a blue oxide of iron. This oxide is produced by the iron of the positive wire, which is partially reduced by the negative wire. It deoxidates itself at first into a green oxide, and afterwards into a blue oxide.

Another interesting product which I obtained is the oxide of silver hyperoxidated. This oxide is formed in any solution of silver, on the side of the positive gold wire of the pile. It has a perfect metallic sound, conducts electricity and galvanism exceedingly well, is very friable, and has an appearance of galena of iron. It often forms lances or columns, slender and straight, of the length of from 3 to 4 inches, and from half a line to three-quarters of a line in thickness. These columns represent in their whole length a succession of uninterrupted crystals, in the form of andreolite, so that each section across the columns gives in some sort the shape of a Greek cross. I should have much to add to describe completely this product; but an experiment, and, if needful, the assistance of the microscope, will afford a much better idea of it than I could give by description. The hyperoxide of silver, reduced to powder, and thrown into simple muriatic acid, produces, even in cold, a very strong effervescence, accompanied with an abundant disengage-

* From *Van Mons's Journal*, No. 17.

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ment of oxygenated muriatic acid gas, and converts itself almost instantly into luna cornea. In employing for this experiment crystals instead of powder, there is heard even at a distance a strong crackling.

You can obtain in the same manner very quickly the brown oxide of lead. This oxide takes the shape of a little scooped out channel. It has a very brilliant metallic appearance, and conducts electricity very well. The brown oxide of lead with muriatic acid, even in cold, also throws up vapours of oxygenated muriatic acid, but not nearly in the same abundance as hyperoxide of silver.

Other metals afforded me similar products, but I have not yet had time to examine them.

In like manner as silver allows itself to hyperoxidate, it allows itself to hydrogenate by the pile. You obtain the hydrogenated silver in decomposing by the gold wire, or by the negative silver of the pile, a solution diluted so sufficiently that the disengaged hydrogen shall be in greater quantity than is necessary to deoxidate the silver. The superabundant hydrogen combines with the metal, and forms a composite of the same colour, which deposits itself under the appearance of a black magma, of a spunge-like body, or of beautiful dendritic forms, and which is the true hydrogenated silver.

Priestley knew this substance, and gave it the name of dephlogisticated silver. Bucholz had also obtained it, and regarded it as silver incompletely reduced. But by heat one may disengage hydrogen under the form of gas; after which there remains reduced silver.

I expect to obtain violent detonations from the mutual reaction of hyperoxidated silver and hydrogen. Copper is hydrogenated under the same circumstances, and takes a blue colour, with the most beautiful and various shades.

The same hydrogenation succeeded equally well in my experiments with tin.

I have not yet examined the black precipitate of gold which is obtained at the wire of the negative pole of the pile. Silver, copper, and tin take the highest degrees of hydrogenation by exposure in a very diluted state to strong piles having gold wires from three-quarters of a line to one line in thickness; and the highest degree of hydrogenation, chiefly for silver, appeared to constitute the gaseous state. We know already the gaseous state of the highest degree of hydrogenation of most other metals.

 X. *Che-*

X. *Chemical Experiments on Mercury. By Messrs.* BRAAMCAMP *and* SIQUEIRA-OLIVA, *of Portugal**.

THESE experiments have for their objects: 1. To ascertain the action of phosphorous acid, of phosphites, and of phosphorus, on the oxides and salts of mercury: 2. The analysis of some mercurial salts, by means of phosphorous acid: 3. To ascertain the action of hyperoxygenated muriatic acid on red oxide of mercury.

1. *Of the Action of Phosphorous Acid upon the Oxides of Mercury.*

Having placed 10 grammes of red oxide in contact with phosphorous acid a little concentrated, the colour of the oxide was changed into a gray. On boiling this mixture, we saw, in a few instants, running globules appear, which led us to suppose that the phosphorous acid had passed into the state of phosphoric acid, by combining with the oxygen of the oxide of mercury; and also that perhaps the phosphoric acid, as it formed, had dissolved some portion of the oxide of mercury. To ascertain this point, we filtered and treated the liquor by sulphurized hydrogen: to our great astonishment, this reagent afforded but extremely slight indications of the presence of mercury. The residuum that remained on the filter, after having been well washed and dried, gave us nine grammes of mercury. Hence we concluded that the red oxide of mercury contains nearly ten per cent. of its weight of oxygen.

The result of this experiment points out new means for analysing the oxides of mercury, which appear to us preferable to sublimation, which is less expeditious, and is attended, besides, with the inconvenience of not giving accurately the quantity of mercury reduced to the metallic state, either because by accident it may be volatilized, or adhere so closely to the vessels employed in the operation that the whole cannot be detached.

We hoped also to be able, by means of this acid, to analyse the salts of mercury, by adding to them potash to decompose them at the same time that the phosphoric acid should reduce the oxide into running mercury; but the results were not satisfactory. Having tried this process on 10 grammes of oxi-muriate of mercury, we obtained only 66 of running mercury, instead of 73, which we ought to

* From *Annales de Chimie*, vol. liv.

have obtained, as we shall show hereafter. This loss of 0·7 is to be attributed in part to what was dissolved by the phosphoric acid which was formed, but still more, in our opinion, to the potash. Mercurial salts decomposed by means of this alkali always retain some of it. M. Berthollet has shown, that during the decomposition of these salts by potash, neither the redundance of the latter, nor ebullition, can entirely clear the solution of mercury.

2. *Of the Gray Oxide of Mercury.*

With phosphoric acid we treated ten grammes of gray oxide, obtained by decomposing by ammonia a solution of a sulphate of mercury at the minimum, strongly boiling the precipitate to dissolve and separate all the triple salt that might have united with it. From these 10 grammes we obtained 9·25 of reduced mercury, and we concluded that this oxide contains $7\frac{1}{2}$ per cent. of oxygen. The mean which we employed for obtaining the gray oxide of mercury seems to us preferable to that of precipitation by caustic fixed alkalies, which combine in part with the oxide precipitated, from which it is impossible entirely to separate them. When, in the method with ammonia, some portion of this adheres still to the oxide, we may easily expel it by a moderate heat.

3. *Of the Action of the Phosphorous Acid upon the Salts of Mercury.*

Having, without success, attempted to analyse the mercurial salts by means of caustic potash, we determined to treat them in a direct manner with phosphorous acid, and we obtained the following results:

1. Phosphorous acid in excess decomposes all the mercurial salts, without exception, reducing their oxides into running mercury, and entirely separating their radicals.

2. When these salts are at the maximum of oxygenation, it causes them to pass into the minimum before it decomposes them.

3. Mercury is completely reduced to the metallic state by this means; for, the oxide of mercury being united to a radical, which does not quit it entirely until it arrives at the metallic state, the phosphoric acid formed in the operation cannot dissolve the mercurial oxide, not being in contact with the mercury until it arrives at the metallic state. The acids, before united to the oxides of mercury, cannot redissolve it while the phosphorous acid which destroys the action is present. Should a phosphate accidentally

dentally be formed, it would immediately be decomposed by the phosphorous acid.

4. *Of the Analysis of the different Mercurial Salts.*

Convinced from the preceding experiments, that the phosphorous acid afforded the best means of analysing the mercurial salts, we attempted the following analyses.

Of Turbith Mineral.

Having boiled ten grammes of this salt, very dry and well prepared, till we saw the mercury reduced, we filtered the whole through gray paper. All the reduced mercury, collected in a single globule, weighed 7·7, which, according to the analysis of the red oxide of mercury, is equivalent to 8·47 of the same oxide. The filteted liquor, treated by the muriate of barytes, gave 5 grammes of sulphate of barytes, which, at 30 per cent. of sulphuric acid, representing 1·5 of this acid. There remains a loss of 3 centigrammes, which may be attributed to the moisture.—This result differs a little from that given by M. Fourcroy.

Recapitulation.

Oxide of mercury at the maximum	-	84·7
Sulphuric acid	- - - -	15·
Loss, probably to moisture,	- -	3
		100

Of Neutral Phosphate of Mercury at the Maximum.

Ten grammes of this salt well dried, treated in the same manner with sulphurous acid, gave the following result:

Red oxide	- - -	63·8
Sulphuric acid	- -	31·8
Loss by moisture	-	4·4

Of Oxygenated Muriate of Mercury of Commerce.

Ten grammes, treated with phosphorous acid, gave of running mercury 7·3, which represents 8·03 of oxide at the maximum. The filtered liquor, treated with nitrate of silver, gave 7·4 of muriate of silver, which represent 1·86 of muriatic acid. This salt being formed by sublimation, contains no water, and we attributed the 11 centigrammes loss to the iron, which is always found more or less mixed with this salt as it is met with in commerce.

 Result.

Result.

Muriatic acid - - -	18·6
Oxide of mercury at the maximum	83·3
Loss attributed to the iron -	1·1
	100

On Nitrous Turbith, Nitrate of Mercury at the Maximum of Oxygen, and at the Minimum of Acid.

Ten grammes of this salt, as dry as possible, treated with the phosphorous acid, gave 8 grammes of reduced mercury, which represent 8·8 of red oxide of mercury: what is wanting to the complement of 10 grammes is to be attributed to the nitric acid, which we cannot collect in this operation, because phosphoric acid, by seizing a part of its oxygen, causes it to evaporate in nitrous vapours. The perfect dryness of the salt leads us to believe that the remainder of the weight may, without fear of error, be attributed to the nitric acid.

Result.

Oxide of mercury at the maximum	88
Nitric acid - - - -	12
	100

Of Phosphate of Mercury at the Maximum.

Ten grammes of this salt, also as dry as possible, treated in the same manner with phosphorous acid, gave 6·5 of running mercury, which correspond to 7·15 of oxide of mercury at the maximum; the 2·85 wanting to complete the 10 grammes, we attribute to the phosphoric acid.

This experiment proves that phosphorous acid not only decomposes all the salts formed by mercury with other acids, but also those which are formed by the phosphoric acid; so great is the affinity of this acid for oxygen, that it surmounts that of the mercury for the same principle, and at the same time the attraction of the phosphoric acid for the oxide of mercury.

This phænomenon also clearly points out the reason why the phosphoric acid, formed at the cost of the oxygen of the mercurial oxides, does not dissolve the mercury as long as any phosphoric acid is present; this decomposes in its turn the phosphate which might be formed.

Of

Of Phosphites.

The phosphites likewise deoxygenate the oxides of mercury, but their action is incomparably less than that of the phosphorous acid. It appears to be subordinate to the force of affinity of the phosphorous acid with its base, and to the action which this may equally exercise upon the oxides of mercury: the phosphites therefore do not seem to us to be capable, in any case, of affording an accurate means for the analysis of these oxides. They also deoxygenate the mercurial salts; but this action is weakened by the same causes.

Of the Action of Phosphorus upon the Mercurial Oxides and Salts.

Pelletier, who has attempted to combine phosphorus with all the metals, says, that on treating the red oxide of mercury with this substance by means of water, in a gentle heat, he obtained a phosphuret of mercury in which the phosphorus seems to exist in a state of feeble combination, and that he obtained by the same operation some phosphoric acid. On repeating his experiment we obtained the same results; but it appears to us that the formation of the phosphoric acid which is produced in it may be differently explained. We are of opinion that the phosphorus (which gives origin to this acid) attracts a portion of oxygen from the atmospheric air, and passes into the state of phosphorous acid. What has led us to form this conclusion is this: The phosphorus which is carried off by the vapours of the water burns at the surface of the latter; and consequently forms there phosphorous acid, which must be changed into phosphoric, in proportion as it seizes the oxygen from the red oxide of mercury. This explanation appears to us the more natural, as phosphorus, placed in contact with the red oxide, does not become acidified, though it deoxygenates the latter, as we shall show hereafter, and as phosphorus boiled in water is changed, by this simple operation, into phosphorous acid.

When phosphorus is placed in contact with red oxide and water in the cold, it first attracts oxygen from the red oxide, and reduces it first of all into gray oxide, and at length to the metallic state; but in this case no phosphorous acid nor phosphoric acid is formed; the phosphorus merely becomes oxidated, and assumes a dark colour. It is not difficult to conceive the theory of this phænomenon: the phosphorus, having great avidity for oxygen, separates it from the oxide with which it is in contact; but this combustion

combustion is so slow, as is proved by the uniformity of the temperature of the liquor during the operation, that the phosphorus is never in a condition to seize upon the portion of oxygen required for its conversion into the acid state. The reduced mercury cannot combine with the phosphorus; for, as it does not combine with it when in the state of fusion (according to the experiments of Pelletier), it is still less capable of combining with it when in a solid state, and without any change of temperature. Our experiment may perhaps furnish the means of obtaining the true oxide of phosphorus, which hitherto has been little or not at all known.

The mercurial salts are equally decomposed and deprived of their oxygen by phosphorus, with the application of heat, or as in the cold; but, in the first case, a phosphuret of mercury is formed, which consequently prevents its analysis by this means. It might perhaps be accomplished in the cold; but the action is so slow, that any other more expeditious means would be preferable.

Of the Action of Oxygenated Muriatic Acid upon the Red Oxide.

This subject has already been investigated by Messrs. Fourcroy and Thenard, and it may easily be supposed that such able chemists have left little to be done in a field which they have already cultivated. We have obtained nearly the same results which they have indicated, and we return to this subject merely in order to notice some slight peculiarities which have escaped them.

Experiment I.

Fifty grammes of red oxide of mercury were put into a proportionate quantity of distilled water. Oxi-muriatic acid gas was then passed through it, taking care to agitate the liquor well, in order that the gas might come perfectly into contact with the red oxide. After the space of an hour the colour of the oxide began to change, becoming darker every moment; we continued to cause gas to enter till the brown powder had deposited itself; we then decanted the liquor, and washed and filtrated this powder, which had become of a deep violet colour: after being dried it weighed 29 grammes. The evaporated liquor presented to us a salt crystallized in the form of needles, which we ascertained by re-agents to be hyperoxygenated muriate of mercury; the last liquor, after the crystallization, presented to us slight traces of another salt more highly oxygenated than the preceding;

ceding; but its quantity was too small to be subjected to any experiment.

The violet powder, which has hitherto been considered as an oxide of mercury, more or less oxygenated, being subjected to different experiments, gave the following results:

1. Boiled in water, it was found insoluble in it, and did not in the least change its colour.

2. Treated with caustic potash, it was converted into red oxide, and the liquor contained muriatic acid. There existed therefore in this powder a muriate of mercury.

3. In order to determine the nature of the latter, and to ascertain the proportion in which it was contained in it, we sublimed 10 grammes of this powder, and obtained 2 grammes of sublimed muriate, and 8 of red oxide not sublimed. The sublimed muriate dissolved almost entirely in the muriatic acid, and the extremely small portion which did not dissolve was mild muriate.

Hence it follows, that the violet-coloured powder which is formed by the action of the oxi-muriatic acid upon the red oxide of mercury, is not a simple oxide of mercury, but an oxi-muriate of mercury, with a great excess of red oxide of mercury, in the proportion of 2 to 8; that this great excess of oxide is combined with the salt; at least this seems to be proved by the first experiment, since boiling water was not able to separate the muriate of mercury from the red oxide.

Experiment II.

From all the circumstances of which we have just given an account, we concluded the possibility of forming a hyperoxygenated muriate of mercury, of a degree superior to that of corrosive sublimate. As the action of the oxygenated muriatic acid had not given in the cold so satisfactory a result as we wished, we boiled 30 grammes of red oxide of mercury with oxygenated muriatic acid, taking care to add fresh quantities in proportion as it was absorbed by the substance. When this refused to absorb any more, the liquor was decanted, and the powder washed and dried; the quantity of the latter was nearly the same as in the former experiment; treated by the same re-agents it gave us similar results, and by sublimation it yielded the same proportions.

The liquor being properly evaporated, it yielded oxygenated muriate of mercury perfectly crystallized. The last portions of the liquor not presenting any appearance of

of crystallization, were evaporated to dryness, and presented to us what we sought for, a hyperoxygenated muriate of mercury, possessing the following properties:

1. It is highly soluble and deliquescent.

2. Much more soluble in alcohol than the ordinary oxygenated muriate.

3. It decrepitates with concentrated sulphuric acid, assumes a yellow colour, and disengages oxygenated muriatic acid gas.

4. The essential property, which no other salt besides this is known to possess, is, that being mixed with the sulphuret of antimony, it inflames spontaneously at the ordinary temperature, some instants after the mixture is made. The residuum of this combustion, besides the sulphuric or sulphurous acids which are disengaged, consists of oxygenated muriate of mercury (corrosive sublimate) and muriate of antimony. It appears, therefore, that, in this case, the superabundant oxygen of the hyperoxygenated muriate burns a portion of the sulphur, and produces sulphuric acid, and a portion also of the antimony, which then combines with some of the muriatic acid from the mercury. This salt, however, possibly on account of its extreme deliquescence, does not decrepitate upon ignited coals, nor does it make any explosion when struck by a hammer.

The oxide of mercury combined in this salt is of the same nature with that which is combined with oxygenated muriate. The alkalies likewise precipitate it in a yellow oxide.

Conclusion from the last Experiments.

Oxygenated muriatic acid produces, therefore, with red oxide of mercury, principally by the aid of heat, different kinds of salts.

1. Muriate of mercury at the maximum, with a great excess of oxide, resembling turbith mineral as to its insolubility in water, but reducible by sublimation into oxygenated muriate of mercury, and into red oxide.

2. Simple muriate of mercury. This salt accompanies in small quantity the preceding salt.

3. Oxygenated muriate of mercury, which crystallizes by the evaporation of the liquor.

4. Hyperoxygenated muriate of mercury. This salt is extremely soluble, and not crystallizable.

Such are the remarks which we had to offer respecting the action of phosphorous acid, and oxygenated muriatic acid, upon the mercurial salts and oxides. Justice requires that

that we should acknowledge the advantage which we have derived in our researches from the practical skill and chemical sagacity of M. Paumier, who has assisted us in our labours, and shared in our solicitude to give them all the requisite accuracy.

Conjecture.

Before concluding, we beg leave to offer a conjecture to which this inquiry has given rise. We feel the less reluctance in presenting it, as it refers to a point which, in the present state of our knowledge, cannot yet be a subject of reasoning in the proper sense of the term.

It is well known that mercury, in the state of oxide or of salt, is employed in medicine for the cure of venereal disorders. The effects of this remedy are well known, but its mode of action is far from being so. Does it act by forming a combination with the principle of the disease, or by yielding to it its oxygen, and being reduced itself to the metallic state? The latter opinion, which has some facts to support it, seems the most probable. The experiments of which we have just given an account, persuade us, that of all the substances which the mercurial oxides and salts may meet with in the animal œconomy, none can take from them their oxygen so easily as the phosphorous acid or the phosphites.

Some perhaps may tell us, that there may exist in the animal liquids, alkalies, or alkaline earths, capable of decomposing the mercurial salts, and separating their oxides. We shall only answer, that the alkalies do not exist in the caustic state in these liquids, and that consequently their radicals cannot by double affinities combine with the oxides of mercury; and that, even though the oxide of mercury should be in this state, a substance would still be required that could carry away its oxygen. It is possible that such substances may be discovered in the human body; but we do not know of any which possesses the property of seizing the oxygen from the oxides of mercury in a degree at all comparable with phosphorous acid and the phosphites. It is known that a large quantity of phosphoric acid is contained in the human body: of this the phosphate of lime which constitutes the bones is a proof. It is easy to conceive the formation of phosphorous acid and phosphites in the human body, since phosphorous acid in reality is nothing else than phosphoric acid with an excess of phosphorus. We conjecture, however, that the formation of phosphorous acid and of phosphites might perhaps be supported by the

the following observations: 1. That the malady in question has its origin in the contact of the fecundating parts: 2. That phosphorus performs a principal part in the functions of reproduction. Pelletier has remarked, that phosphorus is the most powerful aphrodisiac known; analysis has shown that the crystals of the human semen are phosphate of lime. Phosphorous acid and phosphites would therefore vitiate the spermatic liquids, which would not be restored to their natural state until the phosphorous acid and the phosphites, seizing oxygen from the mercurial oxides, should return to the state of phosphoric acid and of phosphates, such as they are found in the state of health.

Note of M. VAUQUELIN.

There is no necessity that we should have recourse to the presence of phosphorous acid or of phosphites in the animal liquids, which is by no means proved, in order to explain the reduction of the salts and oxides of mercury, since almost all the animal humours produce this effect.

XI. *New Method of preparing Alum from Pyrites and Clay. By M.* LAMPADIUS, *Professor of Chemistry and Metallurgy at the School of Freyberg**.

I HAD long been of opinion that the sulphuric acid vapours disengaged from pyrites while burning might be applied to some profitable use. In this process, as is well known, when the combustion is once begun, by applying burning wood or other fuel to the pyrites, they can maintain it by their own sulphur; and a certain quantity of sulphuric acid is formed, which is generally allowed to dissipate itself without producing any benefit.

In a journey which I took in 1799 to the vitriol manufactory of Breitenbrunn, in the district of Johann-Georgenstadt, I had the satisfaction to observe that these vapours were turned to some account. The pyrites were burned in a furnace of a conical form, open at the top, and in the sides of which were apertures, with pipes conducting to a large square chest, filled with pyrites that had been burned and lixiviated, and had thus furnished vitriol. When the pyrites in the furnace are kindled, the aperture at the top is closed. The pyrites still continue to burn; the vapours

* From the *Journal des Mines.*

pass through the pipes and collect in the chest called the *condenser*, where they blend themselves with the lixiviated pyrites. The latter still contain a quantity of highly oxidated iron, which combines with the sulphuric acid, and forms vitriol. When these pyrites have remained a sufficient time in the chest, they are again lixiviated, and more vitriol is obtained from them *. Perfect sulphuric acid would not, in this case, be of the same service as vapours, which are not entirely saturated with oxygen.

This fact revived my ideas relative to the employment of the vapours which are disengaged during the burning of pyrites; but, before I proceed to state the experiment I made on the fabrication of alum, I shall introduce an observation concerning the process employed by M. Chaptal. That chemist burns the sulphur with the saltpetre, as is the practice in the English manufactories of sulphuric acid; he receives the vapours of sulphuric acid, which are disengaged, upon baked clay, and thus forms an artificial ore of alum. Would it not be less expensive to oxidate immediately the sulphur of the pyrites by the atmospheric air? By the latter method two expensive operations would be spared—that of purifying the sulphur, and that of burning it with saltpetre; a substance which is at a considerable price. I admit that, in the process which I am going to describe, all the vapours are not turned to advantage; but if the apparatus be properly disposed, very little will be lost.

I shall first make a remark concerning the state of the pyrites, and of the clay employed. Pyrites merely broken are preferable to those which are triturated and washed, when they are to be piled one upon the other: if pyrites in powder be used in the furnaces, they should be mixed with a fourth part of clay, and hardened and dried in the form of bricks. The arsenic contained in the pyrites is not detrimental to the formation of the alum, because the arsenic, being less volatile than the vapours of the sulphuric acid, is arrested at the commencement of the pipes, where care must be taken not to put any clay. As to the clay, that used by potters may be employed, provided it does not contain too great a quantity of iron.

I directed a reverberatory furnace, four feet long, two and a half wide, and two and three quarters high, to be constructed: the anterior part had an aperture a foot square, by which the pyrites were introduced. On the sides of the

* What M. Lampadius here says is not perfectly correct. Water is made to fall continually, drop by drop, into the chest: this water passes through the heap of pyrites, and keeps it constantly lixiviated.

arch

arch were apertures two inches in diameter, which could be opened and shut at pleasure. At the top of the vault was another, of an oblong form, conducting to a wooden channel a foot and a half wide, but which, on account of want of room, was only twelve feet long, and terminated in a chest three feet in diameter. Such was the imperfect apparatus with which I made my first experiment.

A quintal of pyrites, triturated and washed, were mixed with half their weight of clay, and formed into balls, which were gently dried. Another quintal of clay was likewise formed into balls, which were dried and baked, but only till the clay had lost its unctuosity, and was therefore more proper to receive the vapours of the sulphuric acid. The balls of the pyrites were placed in the furnace, on about a cubic foot of wood, intended for kindling the fire. The aperture in front was closed, and only those on the sides were left open: the balls of clay were exposed in the canal and in the chest to the vapours of the sulphuric acid. The combustion of the pyrites continued fourteen hours, and not a vestige of sulphur was deposited; that substance became entirely volatilized under the form of sulphuric acid*. The wooden channel was too short, as I had expected; the greater part of the vapours escaped; the trees and plants in the garden contiguous to the laboratory withered, and their leaves fell off. I was therefore convinced that the pyrites were completely oxidated in my apparatus. As soon as the operation was ended, the balls of clay were covered with an effervescence of alum, which, mixed with 4 per cent. of alkali, yielded alum.

But as the greatest part of the acid was mixed with the alumine, without being saturated with it, I left the balls in a shed, exposed to the action of the air, from August 2, 1799, till the 3d of April the following year. At the expiration of that time I obtained an earthy mass, entirely covered with an efflorescence, and mixed with sulphate of alumine; being treated in the usual manner, it yielded three pounds and a half of alum.

This experiment convinced me of the possibility of obtaining alum by this process, and that in a very œconomical manner: but, to insure success, the tube should be made much longer than mine. The remaining pyrites may afterwards be employed in the manufacture of vitriol.

* We cannot help here suspecting the accuracy of M. Lampadius. It seems likely that by far the greater part of the vapours would be in the state of *sulphurous* acid. Edit.

XII. *Twenty-*

XII. *Twenty-fourth Communication from Dr.* THORNTON, *relative to Pneumatic Medicine.*

To Mr. Tilloch.

Hinde-street, Manchester-square,
October 21, 1805.

SIR,

In Dr. Rowley's late extraordinary publication against the discovery of the virtuous and illustrious Dr. Jenner, he puts down as one of the madnesses of mankind, their belief of any good as having arisen from pneumatic agency. "Cow-pox mad," and "*air mad*," is an easy mode of aspersing, among the vulgar part of the community, those who wish to become, and are zealous to be, benefactors of mankind: and I appeal for the vindication of my name to the *philosophic* world, before whose tribunal I am feelingly alive; not caring, indeed, as many do, after pecuniary gains, which such attacks are intended to deprive me of. If I and my believers are indeed *mad*, I trust it is the madness of St. Paul, a *learned* conviction of the truth: I shall therefore proceed on with more cases confirming the practice.

Letter from Mr. WILSON, Sadler, to Dr. THORNTON.

Case of Herpes cured by Carbonic Acid Air.

Oxford-street,
October 20, 1805.

SIR,

Having experienced the most striking good effects, I might say wonderful, from the *pneumatic* practice, I am delighted to make the same as publicly known as possible. For twenty-five years I was miserably afflicted with herpes from the crown of my head to the sole of my foot, and had employed Dr. Carmichael Smith, and other gentlemen of the faculty; but my terrible affliction baffled all their skill. I therefore applied to Mr. Varley, and he recommended the trial of the carbonic acid air, confined by means of oil-skin, over the parts affected, an hour each day; and such were the astonishing salutary effects of the air, that in the course of two months this universal disease was removed. It is now above seven years ago, and I have never felt any return since, but enjoy a most comfortable state of health.

I have the honour to be, sir,
Your faithful obedient servant,
MATTHEW WILSON.

Observations on this Case by Dr. Thornton.

1. The herpes is a species of hitherto incurable leprosy, and was confirmed in the habit of this gentleman, having existed, as he writes to me, twenty-five years.

2. The common means failing, the trial of carbonic acid air to the parts affected was a judicious application.

3. If meat is put into fixed air it does not become putrid, and even tainted meat becomes sweet by immersion in fixed air.

4. Fixed air also takes off inflammation; for if a blister be cut, and fixed air be applied, the pain instantly ceases.

5. In this case it evidently promoted action, for after each application a moisture always prevailed; the incrustations were before hard and dry.

6. As the cure has existed seven years, a relapse is not to be dreaded; and this case is, I think, a very striking example of the propriety, not of the "madness," of the application of the pneumatic agents.

I have the honour to remain, dear sir,
Your obliged devoted friend,
ROBERT JOHN THORNTON.

XIII. *A specific Remedy for the Tinea Capitis. By Mr.* JAMES BARLOW.

To Mr. Tilloch.

SIR,

HEREWITH you will receive an account of a specific remedy for the *tinea capitis*; the insertion of which in your valuable Magazine will much oblige

Your obedient servant,
JAMES BARLOW.

Blackburn, Lancashire,
October 20, 1805.

Most practitioners in medicine must have frequently experienced the difficulty of curing this obstinate malady. The intractableness of most children when attempted to be controlled or governed by the accustomed mode of treatment, renders the disease in most instances very difficult to subdue; and the quickness with which the hair of the scalp grows in children, has hitherto almost always rendered every effort to conquer the disease of no avail.

It was from a constant failure under the numerous and diversified remedies which have been recommended by authors in this malignant disease, that I was led to adopt the

 subjoined

subjoined lotion; and I am happy to announce to the public, that by bathing the affected head therewith a few times, morning and evening, and suffering the parts to dry without interruption, the scabs will decorticate and peel off from the scalp, and leave the parts underneath perfectly healed; and this without torturing the patient by either shaving the head or cutting off the hair. I have been in the habit of treating this disease in this manner, and with this application, for the last ten years, and have invariably found it to answer (when duly applied) both in children and adults; and in many inveterate cases even where every other means had been previously used without effect, some of which were of several years standing.

℞. Kali sulphurat. (recens preparat.) ʒ iij.
Sapo. alb. Hispan. ʒ jss.
Aq. calcis ℥ vijss.
Spir. vinos. rect. ʒ ij.
Ft. Lotio pro tinea capitis.

Might not the above remedy for *tinea capitis* be efficacious in relieving that dreadful endemic disease called *trichoma*, or *plica polonica?*

XIV. *On the Decomposition of Alkaline Sulphurets by the Oxides of Lead and of Manganese. By M.* Dize*.

If charcoal be mixed with an alkaline sulphate, and then exposed to a high temperature, the oxygen, one of the constituent parts of the acid, burns the charcoal, quitting the sulphur which served as its radical; with which the alkali then combines, forming what is called an alkaline sulphuret. This combination, which is a result of the decomposition of the alkaline sulphates by the charcoal, is not so easily destroyed as might be supposed, especially when large masses are operated upon. The exposure, frequently repeated, of the alkaline sulphurets to a managed heat, is not sufficient to cause the sulphur to volatilize; for the alkali still retains enough of it to render it improper for certain operations in the arts; and the sulphur during its volatilization is partly deflagrated, and forms sulphurous acid, which combines with the alkali. Thus the operation is rendered more complex instead of being simplified, since we obtain only an alkali mixed with sulphuret and alkaline sulphite.

* From *Van Mons's Journal,* No. 15.

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The addition of carbonate of lime to a mixture of charcoal and alkaline sulphate, as was the practice in the first manufactory of soda, established at St. Denys, near Paris, was not an expensive way of decomposing pretty speedily the sulphuret of soda resulting from the decomposition of the sulphate of soda by charcoal; nevertheless the soda was still contaminated with sulphuret and with sulphite of soda, which might be detected by the smell and by the crystallization. The substitution of iron for carbonate of lime, or these two substances employed together, gave no better result. The whole of the alkali could not be obtained pure till after repeated washings, calcinations, and crystallizations.

Such is the state of our knowledge of the means of purifying in the large way, and without incurring too great an expense, the alkalies which may be prepared by the decomposition of the alkaline sulphates. These difficulties, which appear of little consequence in the operations of the laboratory, become very embarrassing in large manufactories, where the results ought to be equally speedy and easy to be obtained.

Of all these processes the best is that employed in the manufactory of St. Denys, near Paris, and which was published at the time by order of government. This process is still the most simple, and most easy to be executed in the large way; at least it has been judged so according to experience. I believe that the result of my researches upon the decomposition of the sulphurets must add to its perfection, and afford some indication of which use may be made in analogous operations.

I shall not detail all the experiments which have led me to this improvement, but shall now state the means which I employ for decomposing the alkaline sulphurets, or freeing the soda from the last portions of sulphuret and alkaline sulphite which it contains.

After having proceeded to the decomposition of the sulphate of soda, by the mixture of charcoal and carbonate of lime, we lixiviate the crude soda, in order to extract the alkali from it. This lixivium is commonly of a yellowish colour, and exhales the odour of sulphur: if we mix with it some drops of diluted sulphuric acid, sulphur is precipitated, and hydro-sulphurated gas and sulphurous acid are rapidly disengaged.

Were we to evaporate afterwards and crystallize this lixivium, we should obtain crystals of soda, from which diluted sulphuric acid would disengage hydrosulphuret and

sulphurous acid. The rest of the liquor would give soda less pure than the first crystallization.

To purify this lixivium, and free the alkali from the sulphur which it contains in solution, and cause the sulphite to disappear, I add to the ley, while it is in ebullition, a sufficient quantity of well pulverized semi-vitreous oxide of lead. This oxide separates the sulphur from the alkali, and forms by its combination an insoluble sulphuret of lead; the sulphite disappears, and is converted into sulphate. The semi-vitreous oxide of lead loses its red colour, and assumes a deep chestnut or blackish hue, by reason of the quantity of sulphur which the alkali contains. The liquor or lixivium becomes as clear as pure water; dilute sulphuric acid disengages from it only carbonic acid gas, and forms no precipitate: in a word, this lixivium precipitates the nitrate of lead in a white state, and the sulphate of copper is a beautiful green; whilst, previous to the addition of the semi-vitreous oxide of lead, the same lixivium precipitated these two metals of a very dark chestnut colour, that is to say, in the state of sulphuret. Thus the semi-vitreous oxide of lead, in the humid way, carries off the sulphur from the alkali of the soda, and may serve for purifying in the large way, and in a very easy manner, the soda proceeding from the decomposition of the sulphate of soda.

After these experiments, and a calculation of the expense of the semi-vitreous oxide of lead, I substituted for it, with equal success, oxide of manganese well pulverized. This oxide, which is cheaper than the former, presents another advantage: it may be employed several times for the same operation, freeing it first from the sulphur by simple calcination.

The facility with which sulphur is thus separated from soda in the humid way, without engaging it in a new combination, that is to say, leaving it isolated and dissolved in the liquid, induced me to try the same means for decomposing sulphuret of barytes; and my attempt was attended with success.

Decomposition of Sulphuret of Barytes.

In order to obtain barytes in a caustic state, sulphate of barytes is commonly decomposed with charcoal, from which first operation results a sulphuret of barytes soluble in water: this sulphuret is decomposed with nitric acid: distillation separates the nitric acid from the barytes, and this alkali remains pure, and fixed at the bottom of the retort. This process is extremely expensive, on account of the large quantity

quantity of nitric acid which it requires. Barytes will certainly become a very valuable substance for the arts, when we shall be able to procure it at a reasonable price.

The following is the most simple and the most œconomical means which I have discovered, and which I had a long time practised for my own use.

When sulphuret of barytes is made by the decomposition of sulphate of barytes and charcoal, I dissolve it in water; after having let it settle and clear itself, I decant it into a vessel, in which I boil it, adding to it pulverized oxide of manganese, till the liquor has lost its yellow colour, and is become clear and limpid.

In proportion as the oxide of manganese separates the sulphur from the barytes, the odour of sulphur diminishes, and instead of it an alkaline odour is perceived; the taste of the liquor, when all the sulphur is combined with the manganese, has a considerable degree of causticity; as soon as it cools, if too much water has not been employed for the solution of the sulphuret, the barytes crystallizes round the vessel.

Nitrate of lead and sulphate of copper are precipitated from their solvents by caustic barytes, the first in a white, and the second in a blue form: thus oxide of manganese has a stronger affinity with sulphur than either soda or caustic barytes has, since this oxide, at the mere degree of ebullition, decomposes these alkaline sulphurets completely. This process appeared to me simple enough to be applied to the preparation of these two alkalies upon a large scale, so as to afford them at a reasonable expense to the arts in which they may be used.

Since I first perceived that oxide of manganese attracted, in the humid way, sulphur from fixed alkalies and from barytes, this means has been a great assistance to me in getting rid of the alkaline sulphurets in many analytical operations, in which the presence of the alkaline sulphuret embarrassed me, without my requiring an acid to decompose it.

Soda and barytes prepared by this process are very pure, and are obtained at a very moderate expense, especially barytes.

XV. *Process for preparing pure Gallic Acid.* By *M.* Richter*.

Infuse in cold water one pound and a half of gall nuts, previously reduced to fine powder, taking care frequently to agitate the mixture. Pass the liquid through a cloth; add water to the pulp which refuses to go through, and again put it through the cloth, using a press to separate the water. Join the liquors, and with a gentle heat evaporate them, and a matter of a dark brown colour, and very brittle, will be obtained.

Pure alcohol poured on this matter, reduced to a fine powder, acquires a pale straw colour. The deposit infused again in alcohol communicates but little colour to it. The brown residuum now left is composed almost wholly of pure tannin.

Mix the two alcoholic extracts, which distil in a small retort to one eighth. What remains will be almost a solid mass. Pour water to it, and expose it to a gentle heat, and you will obtain a clear and almost colourless solution.

Evaporate this solution, and you will obtain from it very small, white, prismatic crystals. The liquor furnishes more, but they are commonly a little coloured. It is sufficient to levigate them with water to obtain them very white. By this process half an ounce of crystals is procured from one pound of galls; these crystals are extremely light, and consequently occupy a considerable space. They possess the following properties:

1. They are less soluble in water than in alcohol. Dissolved in water they redden tincture of turnsole. They combine with alkaline carbonates, separating from them the carbonic acid.

2. These alkaline gallates form black precipitates in solutions of iron, and likewise decompose all other metallic solutions. But if a solution of pure gallic acid be added to a neutral and perfectly clear solution of iron, no change of colour takes place till the solution of iron is decomposed by the external air, which by oxidating the iron still more, and forming a sulphate of iron of a different nature, more favourable to the combination of the gallic acid with the oxide of iron in excess, produces a black colour. If to a solution of iron you add oxide of iron recently precipitated, you immediately obtain a black precipitate. The same re-

* From the *Annales de Chimie*, vol. lii.

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sult is obtained by bringing oxide of iron in contact with a solution of pure gallic acid.

Iron treated with a solution of gallic acid soon communicates to it a black colour, which, however, can hardly be precipitated from it.

Tannin is soluble in gallic acid, and yields with it a liquor resembling the infusion of galls, and which has the property of instantly precipitating acid solutions of iron black, by a double affinity; the tannin combining with the acid, and disengaging oxide of iron, which unites with the gallic acid.

It results from these experiments:

1. That gallic acid does not separate iron from sulphuric and some other acids, excepting some change of combination takes place in the solutions: in this case the iron, becoming more oxidated, requires a much greater quantity of acid, and the redundant oxide of iron unites with the gallic acid.

2. That when a solution of gallic acid immediately forms a black precipitate in neutral solutions of iron, it is not pure, and commonly contains tannin, which combines with the sulphuric acid, and separates from it the oxide of iron which is dissolved by the gallic acid.

3. That during the preparation of gallic acid, all contact with iron must be avoided, even in the filtering paper, which is rarely free from it, otherwise the acid is discoloured by the iron. It may easily be discovered that the acid contains iron, when, in evaporation, small violet spots are formed in the places where the acid is about to crystallize.

4. That as gallic acid dissolves very readily in alcohol, and tannin scarcely at all, this reagent may be employed to separate them. The alcohol, however, should be very strong; for, if it contain ever so little water, some tannin will be dissolved.

5. Of all the processes recommended for the preparation of gallic acid, this furnishes it in the greatest abundance and of the best quality. For, if we consider in particular that recommended some years ago, which consisted in employing the solution of muriate of tin, afterwards sulphurized hydrogen gas, and lastly alcohol, we may readily conceive, particularly if we have practised it, how defective, expensive, and disadvantageous it must be; and at last it amounts to nothing more than rendering tannin insoluble in alcohol; and nothing is effected excepting that this reagent dissolves gallic acid alone. As to the process which recommends the use of glue, that is quite as bad: you in-

 deed

deed succeed in separating the greatest part of the tannin; but a considerable portion, nevertheless, remains in solution; and it even appears that the liquor which is obtained after the separation of the precipitate formed by the glue is nothing but a combination of glue, tannin, and gallic acid, from which not an atom of gallic acid in pure crystals can be procured.

XVI. *Biographical Sketch of Mr.* George Margets, *Chronometer-maker to the East India Company, and Author of the Longitude and Horary Tables.*

Mr. George Margets was the fourteenth and youngest child of John Margets and Martha Ellis his wife. He was born on the 17th of June 1748, in the parish of Old Woodstock, in Oxfordshire. His father was a wheelwright, to which profession his two eldest brothers were brought up. George, however, being early distinguished by the vivacity of his apprehension, was intended for a commercial employment, and was sent to school for the rudiments of a suitable education: but the death of the old man interrupted this design; and the elder brothers unwilling to be at the expense of accomplishing the purpose of their father, George was obliged to follow the profession of his family.

He was placed by his father at the school of the reverend Mr. Ridding, where his genius was early distinguished above all his fellow-scholars for an assiduity very unusual to boyish years, and especially for an eager interest in the subtleties of arithmetic. At the age of fourteen he was apprenticed to his brother; in which situation he became so devoted to his favourite studies, that he used to rise by three o'clock in the summer mornings to follow his own pursuits, previous to the hour of going to his trade.

About the age of eighteen he constructed a machine which exhibited the different motions of the earth. Of the contrivance and movements of that performance, no means of judging remain; it is only by his own estimation of it, after he had acquired celebrity as a time-piece maker, that it is supposed to have merited preservation. When he had finished it he began to make a clock; but not having enough of brass, and little pocket-money, he was obliged to break up the machine to complete the clock, which is still in existence; and, though a rough piece compared with those accurate chronometers with which he has benefited navigation,

navigation, is exceedingly curious, as it exhibits the diurnal motion of the earth, the progress of the zodiac, the spring and neap tides, with the revolution of the seasons. There is an anecdote connected with the history of this clock that deserves to be recorded. A gentleman one day had occasion to speak with Margets, and not being pleased with his answer to a question, exclaimed peevishly, You are a fool. ‘No, sir,’ was the retort; ‘that I am not a fool I can easily convince you.’ The peculiarity of the reply surprised the gentleman, who immediately asked an explanation; and was conducted to the apartment where the clock hung, and several other little finished and incomplete pieces of mechanism lay scattered. On this incident hinged the success of his future life. The gentleman, surprised by the unexpected display of mechanical genius, spoke of it among his acquaintance, by whom a knowledge of Margets reached the duke of Marlborough, who afterwards, as shall be related, became his patron. About this time, to supply the want of money, that he might obtain materials for prosecuting his favourite pursuits, he painted, during his leisure hours, the names of the proprietors on the waggons and carts of the neighbourhood,—the act which imposed that regulation being then executing. It deserves to be noticed, that although so earnestly attached to the higher mechanical works, such was his general ability, that he excelled all the common wheelwrights at their sole employment, and could, in any given time, perform more work than the best of his brother's journeymen.

After finishing his apprenticeship he remained two years with his brother: his proficiency by this time in the knowledge of clocks and watches had become so well known, that all the intervals of his regular business were employed in repairing the clocks and watches in the vicinity of Woodstock. At the age of twenty-three he went to London, for the purpose of obtaining a regular knowledge of watchmaking; and the duke of Marlborough paid the fee that was demanded before he could be admitted into the shop of an artist. It can easily be conceived, and the sequel will show how justly, that a man so assiduous, and so devoted to that profession, which Margets was now allowed to cultivate, would soon excel all common competitors, and improve and extend the boundaries of his art.

In 1776 he married the eldest daughter of Mr. Bellamy, of Charles-street, Long Acre, by whom he had only one child, that died in infancy. Mrs. Margets is still living.

After

After his marriage Mr. Margets continued to apply to his adopted profession with that perseverance and assiduity which characterizes the attachments of eager and enterprising minds, and which with humbler talents is respected as the virtue of industry. His attention was chiefly directed to the improvement of his machines, which from the peculiarity and elegance of their construction became articles of exportation to India, and were sought after for that purpose with such an increasing demand, that his pecuniary circumstances were in consequence gradually benefited. About the year 1780 or 1781, however, when the political affairs of Hindostan were so much distracted as to affect all interested in the Indian trade, his stock of goods began to swell in his possession, and his customers to diminish; by which he found himself compelled to solicit the assistance of some friends, who very readily granted him a considerable accommodation on his own bond. But unfortunately their friendship was rendered, in the end, of very vexatious consequence to them as well as to him, by the chicanery of an unprincipled attorney, who persuaded them to put the bond in suit, and to sell off the stock of Mr. Margets' shop, the proceeds of which he kept to himself.

Mr. and Mrs. Margets at this time retired to private lodgings, where he persevered in his business with unremitted zeal, occasionally occupying the intervals with the composition of those longitude and horary tables that have proved the speculation of his thoughts to have been superior to mere mechanical ingenuity. His longitude tables were published about the year 1790, and were immediately recognized and acknowledged by the most distinguished navigators and mathematicians. M. de Lalande mentions them several times in very flattering terms of approbation; and several of the boards and public companies of this country patronized and extended their use both by subscription and recommendation. Soon after, he completed his horary tables, which, with the others, form the best assistance to mariners for finding the longitude by astronomical observations that has been offered to the public. Besides these tables, he invented many little mathematical machines, which, as they were rather subjects of amusement than study, are scarcely deserving of particular enumeration. From the epoch of his authorship till the summer of 1804, he had no very important object in view, and continued to enjoy the emoluments of his business and publication with respect, and a growing reputation: indeed, the intenseness of

of his assiduity had so affected his sight, that he was at one time threatened with total blindness, and obliged to sustain an operation on his eyes in 1801.

The circumstances that remain to be noticed are of a most distressing and melancholy nature. Mr. Margets had been all his life, like every other man whose thoughts are employed on abstract objects, or in the adjustment of equilibria in machinery, remarkable for an impatience of interruption in his pursuits, attended with a violence of choler that often rendered him irksome to himself as well as to his acquaintance. On the 27th of June 1804 he was seized by the most deplorable malady that can fall upon the human race. The whole of that day he had been unusually passionate and dissatisfied: in the evening, while he was sitting with Mrs. Margets, and two young ladies who then resided with her, he became calmer, and began to express a kind of contrition for his extravagance: suddenly an apprehension of insanity flashed upon his mind, and he exclaimed, "Good God, is this to end in madness!" and, bursting into tears, continued to weep with his family, whose affliction may be easier imagined than described. His disorder continued to increase, and the lucid intervals to diminish: he was, however, for several days not altogether incapable of business. In this state he forced the family to go with him to Portsmouth, where they continued about two weeks, in the unhappy situation of seeing the ravages of the disease upon his person. At length he was persuaded to return home to London, where proper attendance and advice were immediately procured; but his case was of such a class that Dr. Willis entertained no hope of his recovery. In the course of a month, so entirely had the disease enveloped his intellects, that he had lost the occasional sense of his own distressful condition; and it was thought advisable to send him to St. Luke's, where he would be prevented from committing any violent actions. After he had continued in that hospital about four months he began to refuse his food and medicine, and grew so ill and meagre that it was not expected he could live long. He was therefore taken home. As he approached the period of life his strength decreased; but his mind assumed a more coherent character, and his reason seemed to return. About three days before he died his faculties were restored; his strength, however, was reduced so low that he could not speak without pain. He died on the 27th of December 1804.

J. B. G.

XVII. *Aci-*

XVII. *Acidulation of Sulphate of Potash. By M.* Oersted*.

During my stay at Berlin, I had begun in the laboratory of M. Hermbstadt a series of experiments on the inert or insipid sulphurous acid of Winterl. The sulphurous acid with which I conducted my operations was made after the manner prescribed by Fourcroy and Vauquelin; but in place of obtaining a neutral sulphate I obtained an acidulous sulphate, of which the solution turned blue vegetable colours into red, and made an effervescence with acids. The neutral combination was scarcely crystallizable.

The sulphate of potash could no longer change itself into a sulphite, as Fourcroy and Vauquelin suppose; but a triple salt was formed, composed of sulphuric acid, sulphurous acid, and potash. This salt crystallizes itself in hexangular prisms: it is less soluble in water than the sulphate of potash, but more so than the sulphate of the same alkali; and if treated with an acid more powerful, such as sulphuric acid, it spreads an odour of sulphurous acid gas.

XVIII. *Proceedings of Learned Societies.*

ROYAL INSTITUTION.

The lectures will commence on Monday, the 11th of November, and the several courses will succeed each other in the following order:

Mr. Davy, on chemistry.
Mr. Allen, on natural philosophy.
Rev. T. F. Dibdin, on English literature.
Mr. Landseer, on engraving.
Rev. Sydney Smith, on moral philosophy.
Dr. Reeve, on moral and physical history of man.
Rev. William Crowe, on poetry.
Mr. Opie, on painting.
Dr. Shaw, on zoology.
Rev. John Hewlett, on belles lettres.
Dr. Crotch, on music.
Rev. Edward Forster, on commerce.
Mr. Craig, on drawing.
Dr. Smith, on botany.

* From *Van Mons's Journal*, vol. vi.

LITERARY AND PHILOSOPHICAL SOCIETY, NEWCASTLE UPON TYNE.

This society, in its twelfth year's report, has published a resolution which, if adopted by other institutions, might be productive of mutual advantage. The following is the resolution to which we allude:

"That the subscribers to the public library at North Shields (and to other similar institutions which shall afford an equal accommodation to the members of the Newcastle Society) shall be admitted to the rooms without introduction, on producing to the librarian a certificate of their being members of such institutions."

ORIGINAL VACCINE POCK INSTITUTION.

The following statement was entered by Dr. Pearson on Friday September 27th:

1. That the human animal œconomy and the animal œconomy of cows are capable of undergoing the vaccina once only.

2. That the human animal œconomy is, according to the most accurate evidence, capable of undergoing small pox once only.

3. That the animal œconomy of cows is unsusceptible of the small pox.

4. That the human animal is incapable of taking the small pox subsequently to the cow-pock.

The preceding propositions are only to serve as a foundation for reasoning conjointly with the following propositions.

5. Observations contained in our registered cases show, that the change effected in the human animal œconomy by undergoing the cow-pock or small pox, which renders it unsusceptible of either of these disorders a second time, is not wrought in a moment, as if from an impulse, but it is gradually produced during several days.

The duration of the agency of the vaccine or variolous matter in producing this unsusceptibility seems to be commonly three or four days.

It scarcely or perhaps never commences earlier than the 8th day after inoculation, nor later than the 12th, unless some counteracting agent or circumstance suspend or impede the action of either of the two morbid matters. Of course the duration of the agency of them is for about four days successively, from the 8th, 9th, 10th, 11th, and perhaps the 12th. For brevity and distinction, the time during which the state of unsusceptibility is effected may be called

the *antivariolating process*; and the state so produced may be named the *antivariolous state.*

6. It appears from instances which are registered, that variolous and vaccine matter may be circumstanced so that their respective appropriate agencies upon a given human constitution shall be contemporary; *i. e.* commence their action at the same time. In such instances, a very much smaller proportion of cases of severe constitutional disorder occur than in the singly inoculated small pox; and although eruptions most frequently appear, they are most usually fewer in number, of shorter duration, and defective as variolous ones in the respects of size, in not suppurating, and often not containing infectious matter. Oftentimes there are only mere pimples, and sometimes no eruption at all.

7. The vaccine matter has in many instances been known to exert its constitutional agency at various periods, whilst the *antivariolating process* is going on by variolous inoculation; in which circumstances there was a much smaller proportion of exemptions from eruptions; and although there were some instances of small pox regular and perfect, in all their stages of pimple, vesicle, and pustule, yet there was a very large proportion of slight and defective variolæ, as just stated in proposition 6; and if in certain cases the small pox did not seem to be mitigated and rendered defective, there was no indication that they were in any case rendered more severe. In general, the degree of the variolous disease was directly as the length of time between the inoculation of variolous matter and the subsequent inoculation of vaccine matter, within the limits of space of time in which successive insertions of the two morbific matters can act in conjunction during the *antivariolating process.*

8. The variolous matter also has been made to exert its constitutional agency at various periods of the antivariolating process excited by vaccine inoculation, and then there was a less proportion of eruptive cases: the eruptions were in general small, fewer in number, of shorter duration, and often more defective, in not only having no purulent matter, but even no lymph, than in the set of cases last mentioned (proposition 7.): of course the constitutional illness occurred in a smaller proportion than in the preceding cases.

The principal different circumstances in which the two morbific matters were placed to exert their energies were the following:

1st. The insertion of vaccine and variolous matter into the same punctured or abraded parts.

2d. The

2d. The insertion of vaccine and variolous matter into the vaccine pimple or vesicle already formed.

3d. The insertion of vaccine matter into the variolous pimple or vesicle already formed.

4th. The inoculation of variolous and vaccine matter on distinct parts, but at the same time.

5th. The inoculation of variolous matter at certain determinate times, after vaccine inoculation, in distinct parts, but on the same person.

6th. The inoculation of vaccine matter at certain determinate times after variolous inoculation, in distinct parts, but in the same person.

7th. The inoculation of the two morbific matters contiguously, yet so that either as the vaccine and variolous grew to a certain size, the matters intermingled, or they were mingled by perforating the vesicles to render them confluent.

Remarks.

1. The inoculation with the matter of the vaccine and variolous vesicles existing in the same persons, excited simple regular vaccina and variola in distinct persons.

2. The inoculation with the matter of the vesicle produced by the insertion of both vaccine and variolous matter commonly produced the vaccina without eruptions: sometimes, however, they did occur, but the matter of these eruptions did not in two or three trials excite either vaccina or variola. From the irregularity of the pock of the inoculated part and the eruptions, it seemed reasonable to conclude that perhaps such matter consisted of both vaccine and variolous, which was capable of propagating the two diseases in some instances conjointly. The experiments on this point, however, are considered to be inadequate to answer the question.

3. The trials of the late Dr. Woodville and the reporter, by inoculating with mixed variolous and vaccine matter, proved formerly that no hybrid disease could be formed: but their former conclusion does not now seem just, that in some instances the variolous disease, and in others the vaccine alone, was excited by such matter; nor had they then the least notion of cotemporary agency of the two morbific matters both locally and constitutionally.

4. There will now be no difficulty to explain the instances of breaking out of the natural small pox eruptions between the 8th and 14th day in the vaccine inoculation, without seemingly disturbing the progress of the vaccine

pock,

pock, through its stages of pimple, vesicle, scabbing, and scarring.

5. The evidence from the preceding sources of observation is various, numerous, and uniform, in establishing uniformly the law,

THAT VACCINE MATTER POSSESSES THE PROPERTY OF OPPOSING THE AGENCY OF VARIOLOUS MATTER; BUT VARIOLOUS MATTER HAS NOT THIS POWER AGAINST THE VACCINE;—the counteracting power is not mutual, it is on one side only.

6. Concerning the application to practice, it seems, from the establishment of the law just enunciated, to be a conclusion *à fortiori*, that a constitution in which the appropriate agency of vaccine matter has been exerted, is rendered incapable of having the appropriate variolous agency excited; but,

7. The numerous occurrences of the small pox during the present epidemic variola, in persons who have been supposed to have been vaccinated, prove that the process of vaccination had not been duly excited. These failures afford no objection to the principle, but a great one to the mode of practice; and it is fortunate for the public, though a misfortune to individuals, that the current year has manifested the insecurity of many persons who have been inoculated for the cow-pock, as it may be the means of security for the future.

The above propositions, which are the result of a great variety of trials instituted with patient diligence, confirm the foundation already very strongly established by an infinite number of instances of inoculation in common practice, that it is a law, that the human animal œconomy is rendered unsusceptible of the variolous disorder, by having gone through the cow-pock.

A small proportion of persons have certainly taken the small pox after supposed vaccination; and this it was predicted would be the case long ago, in the "Report of this Institution," and in another work, "The Statement of Evidence;" and it is now asserted, that in a future epidemical small-pox it may reasonably be expected that many more will take that disorder. But the late failures and insecurity of many persons already inoculated for the cow-pock afford no evidence against the efficacy of vaccination; they only manifest,

1. That some years after the new practice the history of vaccine affection was not sufficiently investigated to afford rules of secure practice,

2. That

2. That many inoculators do not appear to have been masters of what was investigated by others, or they had not had sufficient experience to attain requisite skill.

3. That a great proportion of asserted failures were mistaken, either in the patient's not having the cow-pock in the first instance, or the small pox in the second. The reporter's confidence will not be at all shaken, until in his own practice he is a witness to very different cases from those he has hitherto seen; or until professional men, with equal opportunities and equal attention, have stated their adverse evidence to the public. He is persuaded that the subject of the vaccine disorder is now sufficiently investigated for secure practice, and feels himself justifiable in affirming, that in future the occurrence of the small pox after vaccine inoculation WILL BE IMPUTABLE TO THE INOCULATOR BEING UNQUALIFIED, PROVIDED THE PATIENT BE OBEDIENT TO HIS DIRECTIONS.

It is necessary to notice, that the property of vaccine matter in counteracting the agency of variolous matter cannot be determined but by a number of trials in different subjects, because either of the two morbific matters may act merely locally, and in some cases may fail even to produce local effects. The reporter, as on former occasions, is willing, at convenient opportunities, to repeat any of the trials to prove the above results from evidence at the Institution.

Note.—It is a reasonable inference, that if a person has already received the small pox matter into the constitution at the time of vaccine inoculation, there will be not only a chance of the agency of the vaccine matter, so as to anticipate that of the variolous altogether, but also of a coincident agency, by which the small pox will be mitigated. Hence, persons who had been exposed to variolous effluvia ought, of all others, to be inoculated with vaccine matter.

P. S. Having no doubt excited the most interesting curiosity, and perhaps great doubts of the validity of the conclusions in the above minutes,—if required by our readers, we shall ask permission to illustrate them by examples in practice, which are registered at the Institution for insertion in future numbers.

IMPERIAL ACADEMY OF SCIENCES, PETERSBURGH.

The minister of the Russian marine, vice-admiral Tchitchagoff, has sent to the academy a question on the

resistance

resistance of fluids, and its application to naval architecture, which the academy has published in the following terms:

Of the two theories of the resistance of fluids proposed and applied to naval architecture by Don G. Juan, in his *Examen Maritime*, and by M. Romme, in his *Art de la Marine*, it is proposed that one or the other of them, for example, that of Don Juan, shall be corrected and improved to such a degree, as to afford results that shall differ from the results of experiment by so small a quantity as may be practically neglected without sensible error:—Or, if these theories cannot be corrected, that a new theory shall be established and applied to naval architecture, which shall lead to conclusions of the same degree of accuracy;—Or, lastly, if it should be impossible to establish such a theory, it is proposed, that from experiments at least there should be deduced a formula resembling those which have been given by Messrs. Bossut and Prony; and such that it shall be not only more conformable to experiments than those formulas, but that it shall lead as nearly as possible to the conclusions drawn from experiments, even when the formula shall be applied to naval architecture.

For the satisfactory solution of this problem the department of the marine has appointed a prize of 1000 Dutch ducats. Papers will be received till the 1st of July 1806, after which period no memoirs addressed to the academy will be received on this subject, the time appointed being sufficient for those new experiments which the solutions in question render indispensable. The memoirs forwarded to the academy must be written in a distinct legible character, either in the French, English, or Russian language.

The academy requires men of science who intend to make application for this prize, to address their memoirs to the perpetual secretary of that body, before the 1st of July, 1806, and that the writer should clear the post charges as far as the regulations of their respective countries will allow. The customary mode of marking the memoirs with a device or motto, and sending at the same time a sealed letter, having the same device, and containing the name and residence of the author, is also to be adopted in the present instance. The memoirs will be examined by the department of the marine and by the academy; the latter of whom will publish the judgment they shall adopt, and the department of the marine will bestow the prize on that author who shall have satisfied the conditions of the program.

XIX. *In-*

XIX. *Intelligence and Miscellaneous Articles.*

RESTORATION OF THE GREGORIAN CALENDAR IN FRANCE.

By a decree of the French conservative senate, the new calendar is to be abolished, and the old one substituted in its stead. The following is an extract respecting this change from the registers of the senate, of the 9th of September:

The conservative senate, assembled to the number of members prescribed by the 90th article of the act of the constitution, Frimaire 22, year 8;

Having seen the project of the senatus-consultum drawn up according to the form prescribed by the 57th article of the constitution, of Thermidor 16, year 10;

Having heard, on the motives of the said project, the orators of government, and the report of the special commission appointed on the 15th of Fructidor, year 13, decrees as follows:

Art. I. Counting from the 11th of Nivose next, Jan. 1, 1806, the Gregorian calendar shall be employed throughout the whole of the French empire.

Art. II. The present senatus-consultum shall be transmitted by a message to his imperial majesty.

The president and secretaries,
(Signed) Francis (de Neufchateau) president.
Colaud and Porcher, secretaries.

Seen and sealed,
The Chancellor of the Senate (Signed) Laplace.

The report made to the senate, in the sitting of September 9, 1805, by the senator Laplace, in name of a special commission appointed to examine the project of the senatus-consultum in regard to the re-establishment of the Gregorian calendar:

"SENATORS,

"The project of the senatus-consultum which was presented to you in the last sitting, and on which you are going to deliberate, has for its object the restoration in France of the Gregorian calendar, reckoning from the 1st of January 1806. It is not necessary at present to examine which of all the calendars possible is the most natural and the most simple; we shall only say, that it is neither the one we are about to abandon, nor that which we propose to resume. The orator of government has explained to you with great

care their inconveniences and disadvantages. The principal fault of the present calendar is in its intercalation. By fixing the commencement of the year at the midnight which at the observatory of Paris precedes the true autumnal equinox, it fulfils, indeed, in the most rigorous manner, the condition of constantly attaching to the same season the origin of the year; but then they cease to be periods of regular time, easy to be decomposed into days; which must occasion confusion in chronology, already too much embarrassed by the multitude of æras. Astronomers, to whom this defect is very sensible, have several times requested a reformation of it. Before the first bissextile year was introduced into the new calendar, they proposed to the committee of public instruction of the National Convention to adopt a regular intercalation, and their demand was favourably received. At that period the convention returned to good principles; and, employing itself with instruction and the progress of knowledge, showed to the learned a deference and consideration, the remembrance of which they retain. They will always recollect with lively gratitude, that several of its members, by a noble devotion in the midst of the storms of the revolution, preserved from total destruction the monuments of the sciences and the arts. Romme, the principal author of the new calendar, convoked several men of letters; he drew up, in concert with them, the project of a law by which a regular mode of intercalation was substituted for the mode before established; but, involved a few days after in a horrid event, he perished, and his project of a law was abandoned. It would, however, be necessary to recur to it, if we preserved the present calendar; which, being thereby changed in one of its most essential elements, would present the irregularity of a first bissextile placed in the third year. The suppression of the decades made it experience a more considerable change. They gave the facility of finding every moment the time of the month; but at the end of each year the complementary days disturbed the order of things attached to the different days of the decade, which then rendered administrative measures necessary. The use of a small independent period of months and years, such as the week, obviates this inconvenience; and already that period has been re-established in France; which, since the highest antiquity, in which its origin is lost, circulates without interruption through centuries, mingling with the successive calendars of different nations.

But the greatest inconvenience of the new calendar is the

the embarrassment which it produces in our foreign relations, by insulating us in that respect in the midst of Europe; which would always exist, for we ought not to hope that this calendar can ever be universally admitted. Its epoch relates merely to our history: the moment when its year commences is placed in a disadvantageous manner, as it participates in and divides between two years the same operations and the same labours: it has inconveniences which would be introduced into civil life, as the day begins at noon, according to the usage of astronomers. Besides, this custom would relate only to the meridian of Paris. In seeing others reckon the longitude from their principal observatories, can it be believed that they would all agree in referring to the commencement of our year? Two centuries were necessary, and the whole influence of religion, to cause the Gregorian calendar to be generally adopted. It is in this universality, so desirable and so difficult to be obtained, and which it is of importance to preserve when it is acquired, that its greatest advantage consists. This calendar is now that of almost all the nations of Europe and America: it was a long time that of France: at present it regulates our religious festivals, and it is according to it that we reckon our centuries. It no doubt has several considerable defects. The length of its months is unequal and whimsical, the origin of the year does not correspond to that of any of the seasons; but it answers very well the principal object of a calendar, by being easily decomposed into days, and retaining nearly the commencement of the mean year at the same distance from the equinox. Its mode of intercalation is convenient and simple. It is reduced, as is well known, to the intercalation of a bissextile every four years; the suppression of it at the end of each century, for three consecutive centuries, in order to re-establish it at the fourth; and if, by following this analogy, we still suppress a bissextile every four thousand years, it will be founded on the true length of the year. But in its present state, forty centuries would be necessary to remove, only by one day, the origin of the mean year from its real origin. The French mathematicians, therefore, have never ceased to subject to it their astronomical tables, become, by their extreme precision, the base of the ephemerides of all enlightened nations.

One might be afraid that the return of the old calendar would soon be followed by the re-establishment of the old measures. But the orator of government has taken care to dispel that fear. Like him, I am persuaded, that instead

of

of re-establishing the prodigious number of different measures which prevailed in France and shackled its interior commerce,—government, fully convinced of the utility of an uniform system of measures, will take the most effectual means for accelerating the use of them, and for overcoming the resistance still opposed to it by old habits, which are already disappearing every day.

From these considerations your commission unanimously proposes the adoption of the senatus-consultum presented by the government.

EARTHQUAKE AT NAPLES.

Naples, August 9.—The duke d'Ascoli, the minister of the police, has transmitted to government the following report:

"The terrible earthquake which was felt in this capital, and the provinces of the kingdom of Naples, on the 26th of July, at ten in the evening, has occasioned, according to the reports which have hitherto been received here, the following devastation and damage:—In the capital, several houses, churches, and convents, have been thrown down: a woman was buried under the ruins of the palace of Corsigliano: 470 buildings have greatly suffered, and threatened to fall down. The palace of Caserta, and a number of private houses, have suffered nearly in the same proportion: a woman in them was killed. At Nola, a part of the barracks belonging to the cavalry, and several houses, sustained great damage; but no lives were lost. At St. Mary of Capua (a fortress in the neighbourhood of Capua), a part of the barracks belonging to the cavalry was almost entirely thrown down: eleven soldiers were killed, and thirty-four severely wounded. At Nevano, and the environs, the inhabitants suffered very little. The town of Isernia is almost entirely fallen to ruins, and more than a thousand people were found dead in it: a part of the inhabitants were saved by betaking themselves to flight. During these dreadful shocks, which convulsed every thing, flames were observed to issue from the earth throughout an extent of several leagues. A small part of the town sustained no damage. In the same province in which Isernia is situated, Campo-Basso, Cerreto, Baraniello, and about seven other places, sustained a fate almost similar; but the number of the dead is not yet known. In the town of Montefusco all the houses were damaged; the bell of the collegiate church fell down; a woman was killed, and several other persons were wounded. The town of Avellino shared the

same fate: some persons and several seminarists perished. The town of Chieti suffered very little: some houses only were damaged. At Salerno the earthquake was strongly felt; but it occasioned very little loss. At Finocella and Mola di Barri, in Apulia, nearly the same scenes took place. In the districts around Vesuvius the earthquake was less felt than elsewhere; the air having found a way to escape through the volcano."

"*Naples August* 10.—Mount Vesuvius has lately made such an eruption, that one similar to it has not been witnessed for a long time: the lava ran down in such quantity, and so rapidly, from the summit of the mountain, that in less than two hours it reached the sea. In its course it divided itself into three parts, which ran between the Torre del Greco and the Torre del Annonziata: on this account it occasioned less damage than if it had fallen in one current on these two towns. It is a lamentable spectacle to see the vines, the corn-fields, and the houses swallowed up in a sea of fire, and covered to the depth of three toises with iron ore; for the lava which ran down on this occasion had absolutely that character. The queen, who was at Castelmare, was not able to return to Naples by land; for the road is covered with three causeways of lava, the least of which is three hundred paces; and it was also impossible for her to go to the theatre of St. Charles, to see the representation given on her birth-day. It is much to be feared that this country will experience the fate of Herculaneum and Pompeii."

Extract of another Letter.—"After the terrible earthquake of the 26th ult. we began to resume a little security; but subterranean noises being heard from Vesuvius, an eruption of the volcano was apprehended, and the inhabitants of Torre del Greco and l'Annonziata removed from their houses, and thought of providing for the safety of their persons and valuables. About a quarter past two in the afternoon Vesuvius made an extraordinary eruption through the same mouth which gave a passage to that in 1794. The latter was even more considerable, having thrown out a current of inflamed lava, which proceeded with great rapidity as far as the plain, over a space of about four miles, and then took its course towards the sea, which it reached in about nine hours.

"It was observed that from its origin this current divided itself into two branches; one in the direction of Portici, which luckily then turned aside, and, uniting itself to the other, formed in the middle a kind of island of boiling lava,

which

which was swallowed up in the sea; where there was seen suddenly formed and raised up, a kind of promontory of volcanic matters. For about twenty minutes the whole extent of the ground occupied by the lava continued in flames, exhibiting, as we may say, a terrible but astonishing spectacle; especially as the inflamed trees presented the aspect of white flames in contrast with those of the volcanic matters, which were red. The lava, as has been said, proceeded to the sea in this manner with great rapidity, carrying along with it enormous masses; and nothing was seen in a great extent of the coast but boiling foam, and eddies of water and of fire. Several persons who were at Portici betook themselves to flight in boats before the torrent of fire mixed itself with the waters. Happily the habitations experienced no damage, and we have not learnt that any person perished during this fatal event."

Letters from Naples of August 2d estimate the loss sustained in that city by the earthquake of July the 26th at twenty millions of ducats. They add the following particulars, taken from the notice to the Neapolitan government by the commissioners sent to the spot:—"At Isernia, where the commotion was so terrible, the earth opened and vomited out flames; 339 families were swallowed up. At Castel Petroso 13 families perished: at Massino 84: at Tresolone, 393: at Saint-Angelo-in-Colla, 43: at Baramillo, 180: at Cantalupo, 142. Lorenzano and Saint-Angelo-di-Lombardo are entirely destroyed. A small river which flowed through that province, and which traversed an extent of fifty leagues, was lost at the distance of four leagues from its mouth."

VOYAGES AND TRAVELS, AEROSTATION, &c.

A letter from Petersburgh, dated August 3, says, "We have lately received some further account of the progress of the expedition commanded by captain Krusenstern. He arrived safe at Japan, where he met with a favourable reception; and the ambassador, M. Resanof, entertains hopes that his mission will be attended with the best success.

"The academy of sciences has purchased the philosophical apparatus of M. Robertson, which is remarkable for the correctness of the instruments, and particularly those destined for electricity and galvanism.

"The aërostatic experiment which M. Robertson intended to make at a great distance from Petersburgh, has been changed on account of the bad weather into a common

ascent.

ascent. The wind, which in the morning had a favourable direction, carried the aëronauts with great violence towards the Baltic. They set out from the academy of arts at half after eight, and terminated their voyage at the mouth of the Neva, at about fifty paces from the sea. When M. Robertson descended with his pupil, he resolved to ascend alone in order to cross the gulf; but the peasants having drawn the ropes of the balloon too strongly to one side, the netting slipped from it, and therefore it was impossible for him to carry his design into execution. His imperial majesty was present at this ascent."

ASTRONOMY.

Table of the right Ascension and Declination of Ceres and Pallas for November 1805.

	Ceres.					Pallas.				
	AR.			Decl. N.		AR.			Decl. S.	
1805	h	m	s	°	′	h	m	s	°	′
Nov. 1	7	23	0	23	47	5	16	32	26	8
4	7	24	24	23	56	5	15	52	27	1
7	7	25	36	24	5	5	14	56	27	51
10	7	26	32	24	16	5	13	44	28	38
13	7	27	12	24	27	5	12	20	29	30
16	7	27	36	24	40	5	10	36	30	4
19	7	27	48	24	53	5	8	44	30	43
22	7	27	40	25	7	5	6	36	31	17
25	7	27	16	25	22	5	4	20	31	47
28	7	26	36	25	38	5	1	56	32	13
Dec. 1	7	45	40	25	54	4	59	24	32	34

ZINC.

In our List of Patents, page 95 of our last volume, we mentioned one granted to Messrs. Hobson and Silvester, of Sheffield, for a method of manufacturing zinc. The discovery of these gentlemen is curious. They have found that zinc, at a temperature between 210° and 300° of Fahrenheit, is not only very malleable, but may be passed through rollers or drawn into wire. Zinc does not return to its former partial brittleness after being thus wrought, but continues soft, flexible, and extensible, and may be applied to many uses for which this metal was before thought unfit.

PURE

PURE CERUSE.

M. Van Mons states, that if lead ashes be dissolved in a sufficient quantity of dilute nitric acid, assisted by a gentle heat, and the solution be filtered, and then precipitated by chalk brought to an impalpable powder by levigation, the precipitate, when washed and dried, will be the purest and most beautiful ceruse possible.

DEATHS.

On Tuesday, the 24th instant, died at his house in Great Titchfield-street, in the 63d year of his age, Mr. William Byrne, a distinguished landscape engraver. He was educated under an uncle, who engraved heraldry on plate; but having succeeded in a landscape after Wilson, so as to obtain a premium from the Society for the Encouragement of Arts, it was regarded as the precursor of talent of a superior order, and he was sent to Paris, at that time the chief seminary in Europe for the study of engraving, for improvement. In Paris he studied successively under Aliamet and Wille; from the former of whom he imbibed the leading traits of that style of engraving which he afterwards adopted as his own: under the latter he engraved a large plate of a storm, after Vernet; but the manual dexterity of Wille was alien to his mind, and probably contributed not much to his improvement, though he always spoke of Wille's instructions with respect.

When he returned to England, the success of Woollett as a landscape engraver had set the fashion in that department of the art; but Byrne disdained to copy what he did not feel; perhaps scorning the influence of fashion in art, preserved the independance of his style, and continued to study, and to recommend to his pupils, Nature, Vivares, and the best examples of the French school.

His larger performances are after Zuccarelli and Both: but his principal works (containing probably his best engravings) are the Antiquities of Great Britain, after Hearne; a set of Views of the Lakes, after Farington; and Smith's Scenery of Italy. His chief excellence consisting in his aërial perspective, and the general effect of his chiaro-scuro, he was more agreeably and more beneficially employed in finishing than in etching; and hence he generally worked in conjunction with his pupils, who were latterly his own son and daughters. His manners were unassuming, his professional industry unremitting, and his moral character exemplary. He seldom went from home, but lived in the bosom of a numerous and worthy family, who are now deploring their loss.

The

The death of the celebrated Klaproth, of Berlin, has been announced in some of the foreign journals. We are happy to state that this intelligence is not correct. He enjoys good health, and is now in his sixty-second year.

M. Justus Klaproth, professor of jurisprudence in the university of Göttingen, well known by the learned works which he has published on that subject, died on the 10th of February last, in his seventy-seventh year.

M. Alexander Saverien, engineer of the French marine, died on the 28th of May last, in his eighty-fifth year. He has been long known to the scientific world by his writings on navigation, and the theory of building, rigging, and manœuvring of ships; accounts of instruments for making observations at sea; his marine dictionary; a dictionary of the mathematics; a dictionary of architecture; history of modern philosophers, and history of the progress of the human understanding. For many of his latter years he was poor and infirm, and was much indebted to the cares of a servant who attended him from attachment. He has left a widow likewise in want, and very aged.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Nyren, of Bromley, in the county of Middlesex, muslin bleacher and tambour worker; for printing fancy patterns on silk and cotton lacenet, instead of tambouring or working them in colours. Dated September 27, 1805.

To Stephen Clubb, of Colchester, in the county of Essex, millwright; for an improved mangle. Dated September 27, 1805.

To James Macnaughtan, of Great Queen-street, Lincoln's Inn Fields, in the county of Middlesex, ironmonger; for a stove or grate and range upon a new construction, by which rooms will be much more effectually warmed than they now are, and the chimneys prevented from smoking. Dated September 27, 1805.

To John Syeds, of Fountain Stairs, Rotherhithe Wall, in the county of Surrey, mathematical instrument maker; for a steering amplitude or azimuth compass and scale for finding and working the course of ships at sea. Dated October 7, 1805.

To Daniel Desormeaux, of Barking, in the county of Essex, surgeon and apothecary, and Samuel Hutchings, of Ilford, in the said parish of Barking, weaver; for their improvements in the making and manufacturing of wax, spermaceti, and tallow candles. Dated October 22, 1805.

METEORO-

METEOROLOGICAL TABLE

BY MR. CAREY, OF THE STRAND,

For October 1805.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Sept. 27	46°	64°	55°	30·22	35°	Fair
28	54	63	54	·40	22	Fair
29	50	60	49	·60	31	Fair
30	46	59	53	·50	21	Cloudy
Oct. 1	49	59	53	·40	25	Cloudy
2	52	63	55	·30	32	Fair
3	51	60	52	·21	35	Fair
4	49	60	49	·25	37	Fair
5	46	61	46	·32	46	Fair
6	41	56	44	·32	15	Fair
7	39	58	47	·28	32	Fair
8	47	63	48	·05	12	Fair
9	50	59	50	29·86	17	Cloudy
10	49	54	40	·55	6	Rain
11	39	51	39	30·04	27	Fair
12	34	51	44	29·96	26	Fair
13	45	50	42	·65	0	Rain
14	42	56	48	·52	10	Showery
15	46	54	46	·42	28	Fair
16	44	52	41	·19	10	Cloudy
17	39	46	42	·51	0	Rain
18	38	49	39	·92	24	Fair
19	34	51	41	30·14	28	Fair
20	39	52	48	·20	10	Fair
21	45	53	47	·14	14	Foggy
22	48	52	44	29·93	30	Fair
23	44	52	46	·70	15	Fair
24	46	54	46	·56	10	Cloudy
25	49	58	52	·46	7	Cloudy
26	50	54	50	·35	5	Cloudy

N. B. The barometer's height is taken at noon.

XX. *Account of the Goats of Angora: in a Letter from M.* Chancey, *to M.* Pictet, *of Geneva**.

The kind of goat which is known in Europe by the name of the Goat of Angora, is not the only one which exists in Natolia and in the environs of this city. We find there also another kind, more common and more resembling that of Europe. Travellers have imperfectly described those two races, which are perfectly distinct; and hence arises the uncertainty that prevails in Europe respecting the products of each. We cannot remove this uncertainty but by a positive description of each of the two races. This distinction will prevent us in future from confounding the short wool of the one kind, which is somewhat like cotton, with the long and silky hair of the other. The two races of goats in the neighbourhood of Angora are known by the names of *kara-gueschy* and *tistik-gueschy*.

1. The *kara-gueschy* or *scys* (or black goat) is the common goat, resembling that of Europe; and is found in Syria, Natolia, and all the East. Its fleece is black, or of a deep brown. The hair is long and straight, sufficiently fine at the end which is fixed in the skin, but more black and coarse at the other end. The kara-gueschy is shorn every year. His hair is of a grosser quality, and is not exported abroad, but employed in these places for making coarse stuffs for tents, and for sacks like our hair sacks. This fleece of Angora is not more valued than the other goat fleeces of the East. Its value in these places is thirty paras for the *ocque* of 400 drachms. Under this hair, and upon the very skin of the animal, there is another fleece finer and shorter. It is composed of slender hairs, of which the length varies from an inch to an inch and a half. These, by their mixture at the root of the longer hairs, form a short wool of a cotton-like substance and of a yellowish gray colour. This part of the fleece, which is by far the most precious, is obtained by throwing water saturated with lime upon the side of the animal while it is yet covered with hair. In a few minutes the hair and the down detach themselves from the skin, and afterwards are easily separated from each other.

The wool of the kara-gueschy is imported rough into Europe, where it is known by the name of Goat's hair. It is employed in different manufactures, particularly in hat-

* From *Bibliotheque Britannique*, No. 193.

hat-making. It is for this use that Marseilles still continues to draw a great quantity of this article; and it is to this city a considerable branch of commerce, and one of the principal articles of return for our manufactures which are consumed in the East.

The wool of this Goat is less abundant in Syria, and its quality is not esteemed. A much greater quantity is drawn from Angora, from Erzerone, and from the north of Persia. The province of Kerman furnishes the most beautiful kind. In general, all these wools are forwarded to Smyrna by the caravans of camels which go from Erzerone. From Smyrna they are forwarded to Marseilles and in the ports of Italy by way of sea.

The art of spinning the wool of the Goat is known only in Syria and Natolia: except in those two places, it is put to no manner of use. Its value in these places arises only from the demand for it from Europe. At Angora the average value is from four to six piastres the ocque of 400 drachms.

The wool of this Goat is also exported rough into Europe from Persia and the province of Kerman; but it has there an intrinsic value from the use to which it is put. The Persians know how to spin it. They make shawls of it similar to those of India, but far inferior in fineness and taste of workmanship. It seems certain that the primary material which is employed for the fabrication of the shawls of Cachemire is also the fleece of a goat similar to the karagueschy. But this fleece is much finer and more precious than any that is imported into Europe. I do not even believe that it is known in Europe: at least, it has never been imported by way of Aleppo, or the other cities of the East.

The Tistik-Gueschy.

The *tistik-gueschy*, or woolly goat, forms the second kind of those animals which is found in Angora. But, instead of resembling the goat of Europe as the kara-gueschy does, this breed is different in many respects. It forms in the genus a decided variety, perhaps even a distinct species. The tistik-gueschy is the goat which Buffon has described under the name of the Goat of Angora. Its fleece is of a clear whiteness. Its hair is long, thin, silky, naturally curled: it is extremely fine, and unlike the hair of the karagueschy, which is as hard as its skin; it is as supple and delicate as the finest wool of the Spanish Merinos. These long and curled hairs compose the whole fleece of the

tistik-gueschy, which is as fine at the top as at the roots, and which are not mixed near the skin with any other soft wool or down. This wool or down of which we speak belongs exclusively to the kara-gueschy breed, and is entirely a stranger to the real goat of Angora.

This difference alone furnishes an obvious mark of distinction between the two breeds. There are, besides, many others, of which one is, that the kara-gueschy multiplies its species throughout all the East, while the tistik-gueschy is confined to the soil of Angora. It is found only in this city, or its environs to the distance of 15 leagues. At a greater distance the race is bastardized; the wool becomes coarser, and the value of the animal is much inferior to that which constitutes the riches of the city from which it takes its name.

The territory of Angora is composed of mountains a little elevated, on which the snow generally lies for two months of the year, and which afford numerous springs of fresh and wholesome water. The rivulets which are thus formed fertilize the soil, which is covered with pasture grass. As soon as the cold has abated they conduct the tistik-gueschy to these mountains, where they pass the mild season, changing their pasturage in rotation every day, and continually exposed to the air. It is only on the winter nights that they are allowed houseing in sheepfolds.

The goats of Angora graze in herds of from two hundred to eight hundred in number: the females and bucks mixed indiscriminately. The latter are higher and stronger than the former; their hair is like theirs, white and curled, but not so fine. The flesh of the tistik-gueschy is better than that of the ordinary goat. It is killed for consumption after five years of age; for at this age the hair becomes coarser, and the fleece is less esteemed.

The tistik-gueschy is shorn yearly, after being bathed in running water. Their hair is clipped with long iron scissars. The fleece of the females, which is more esteemed than that of the males, weighs from 350 to 400 drachms. Their fleeces are all spun on the spot; and it is a remarkable fact, that the place consumes their entire product, without allowing any exportation. The reason is, that it is to this manufacture the inhabitants of Angora owe their subsistence, and they are jealous of preserving it.

Nothing is more simple than the process employed at Angora to work the wool of the tistik-gueschy. As soon as the animal is stript, the fleece is combed with a long iron

comb, of which the teeth are very thickly inserted. The hairs, when thus combed, are clear, and disengaged from all extraneous particles which have adhered to the body of the animal.

All the inhabitants of Angora whom I have consulted have assured me that this is the only operation which the hair undergoes; after which it is clean, and fit for being spun,—an operation with which women are generally intrusted. They spin the fleece on a distaff like that which is used for cotton; sometimes twisting a number of hairs into one thread; but sometimes only three and even two hairs to a thread. This last method of spinning produces thread so very fine and clear, that it is sold for twelve parats the drachm. The price of the other kinds of thread diminishes from this value to that of two or even one parat per drachm.

The hair of the tistik-gueschy, though thus spun, is yet unbleached, and goes through no operation of dyeing. In this state it is sent to the loom, and made into a stuff known in the East by the name of the schallet of Angora. These schallets, of which there is so great a consumption, are all of them the actual manufacture of this city.

It is reckoned that in Angora there are more than two thousand looms in constant employment, and each loom employs from eight to fifteen workmen. This source of wealth, the only one that Angora enjoys, must necessarily be very fruitful, since it yields not even to the destructive influence of that government which has turned to miserable villages so many cities once so flourishing.

The schallet is made in pieces of 28 piks in length and 2-3ds of a pik in breadth, which are sent to dyeing as they come from the loom. They are dyed all manner of colours with every possible variety of shade; but lively reds and violets are most esteemed. The schallet is much superior to the camlet of Europe for the lightness and fineness of its grain, and is much higher priced. The most common sells at fifteen piastres the piece, the dearest at fifty. This latter kind is chiefly consumed in Constantinople and in Egypt.

The beauty and fineness of the tistik-gueschy is a sufficient motive for the experiments that have been proposed for breeding this animal in France. Already some individuals of the breed have been sent to Rambouillet, where they still continue. But hitherto the owners have not been able to make use of their fleeces. Perhaps the above details may be of use for this purpose.

The

The price of the tistik-gueschy at Angora is from 10 to 12 piastres for the females, and from 12 to 15 for the males. We might easily obtain a little flock. The journey to Aleppo would take from 20 to 24 days in the fair season. From Aleppo it would be proper to send the herd to Lattakreh, and from thence to Cyprus, where vessels for France are always to be found.

It would be necessary for the success of this experiment to make some peasants of the country accompany the flock. They are to be hired at Angora. Their payment, which however it is difficult to calculate exactly, will be altogether more than a thousand piastres per annum.

After having showed the facility of an experiment which might eventually propagate in France a precious breed of animals, and which is not to be found in Europe, it remains to obviate an objection which will undoubtedly be advanced, but which it is of consequence not to leave accredited.

We have seen above, that it is to the salubrity of the waters and the nature of the soil of Angora that the people of this country attribute the fineness of this animal's fleece. In fact, at the distance of more than 15 leagues from Angora this race is not to be found, either on the side of Erzerone or in the other parts of Natolia. The very animals which are sent there degenerate. It might seem, therefore, that it would be impossible to preserve the breed in France. But it is easy to answer this by a recent example, and one actually well known in France: I mean the Merinos of Spain. Who can doubt that suitable care and attention would produce on the goats of Angora the same effect that they have produced on this precious race of animals?

In fact, the same prejudices which prevail at Angora existed and still continue in Spain. The proprietors and the majoros di domos are all persuaded that the pure race of the Merinos belongs exclusively to their soil. They are assured, besides, that the purity of the breed is owing to the continual journeys which their flocks make from the mountains of Leon to those of Andalusia. From this conceit arises the facility with which they allow the breed to be imported for propagation into France. This opinion appears also to be verified in the places themselves; for the Merinos which settle at Segovia, known by the name of *piarras*, degenerate from the first year, and their wool loses there in value from 20 to 25 per cent.

But the consequence drawn from this fact against the

possibility of preserving the breed pure in France is absolutely false; for who does not know that the flock of Rambouillet is equal in beauty of fleece, and superior in size and strength, to the finest specimens from the Spanish cacannos?

XXI. *Description of a Machine by which all the Thread-work in Shoe-making may be done in a standing Posture. By Mr.* THOMAS HOLDEN, *of Fettleworth, near Petworth, in Sussex**.

"FROM the sitting posture used in my employment, as a shoemaker, I suffered so much in my health, and from the piles, that I thought I must either give up my business or lose my life. In this difficulty I invented this machine, got it made, and went to work with it. I found it answer to my satisfaction, and its use followed by a restoration of my health. I believe I have made eighteen hundred or two thousand pair of shoes with it, and still work on. I recommend it as the quickest way of closing all the thread-work.

"My machine is fixed to the floor, a little to the left of the seat, but within reach of the hand; the work is held on with a stirrup, and suits to the place."

Certificates from John Summersell, cordwainer, and overseer of the parish; Richard Hawkins, John Tilly, and George Hawkins, Thomas Tilly, and Edward Hawkins, cordwainers, confirmed the above statement; as well as the following letter from Mr. Peter Martin, surgeon, at Tulborough:

"I am sincerely of opinion, that Thomas Holden's invention is a desirable acquisition to men of that profession, especially to those who may be diseased internally, or who may suffer from stomach weakness and indigestion. These diseases may be aggravated, if not occasioned, by their working in a bent posture.

"The inventor, about twenty years ago, often applied to me for relief from a train of bowel complaints, and frequently had occasion to take the medicines usually employed for the relief of dyspepsia.

* From the *Transactions of the Society of Arts*, &c. vol. xxii. who awarded to Mr. Holden a bounty of fifteen guineas for this invention.

"I re-

"I repeatedly informed him, that his employment was the cause of his disorder, and desired him to relinquish it, or invent some method to do his work standing. This hint, and his corporeal sufferings, prompted him to the invention. That it answers the purpose, I have reason to believe, as he and others use it. He is now free from complaints, and so improved in his corpulence and countenance, that he is not like the same man, and for years has had no occasion for medicine."

See Plate IV.

A, the bed for the closing-block, and to lay the shoe in, whilst sewing.

B, the closing-block.

C, a loose bed to lay the shoe in whilst stitching; the lower part of which is here exhibited reversed, to show how it is placed in the other bed A.

D, the hollow or upper part of the loose bed C, in which the shoe is laid whilst stitching.

E, a table on which the tools wanted are to be laid.

F, an iron semi-circle, fixed to each end of the bed A, to allow the bed to be raised or depressed. This half circle moves in the block G.

H, another iron semi-circle, with notches, which catch upon a tooth in the centre of the block, to hold the bed in any angle required. This semi-circle moves sidewise on two hooks in staples at each end of the bed.

I, the tail or stem of the bed A, moving in a cylindrical hole in the pillar, enabling the bed to be turned in any required direction, and which, with the movement F, enables the operator to place the shoe in any position necessary.

K, the pillar, formed like the pillar of a claw table, excepting the two side legs being in a direct line, and the other leg at a right angle with them.

L, the semi-circle H, shown separately, to explain how it is connected with the staples, and how the notches are formed.

M, the tail or stem of the bed A, and the lower part of the bed N, shown separately, to explain how the upper part of the bed is raised or depressed occasionally.

XXII. *An Essay on Commercial Policy. By* J. B. GALT, *Esq. London**.

FREEDOM of execution is so essential to mercantile speculations, that every legislative interference which would limit their objects, or control the modes of their accomplishment, is of greater injury and wider consequence than is generally imagined. The famous reply, "Leave us to ourselves," which the French merchants made to the minister who was desirous of promoting their prosperity, has been often quoted for its wisdom: but though sufficient data be presented in the history of all commercial countries, and particularly in the history of our own, to demonstrate the value of liberty to trade, no regular attempt has yet been made to place the subject in a clear and distinct light. To exhibit such a demonstration, it would be necessary, in the first place, to consider the natural tendency of commerce; and, in the second, how far it should be restrained by political circumstances. Were this done, and a series of facts, linked to certain special laws, produced, the pernicious effects of governments attempting to regulate the objects of trade would be evident, and a degree of certainty on that point of political œconomy, proportioned to the evidence, would be obtained only inferior to mathematical truth. For all special acts of authority relative to trade are boons granted either to individuals, or corporations, or provinces.

The nature of trade has a tendency to blend the interests of mankind together, and to disseminate throughout the whole species a principle of mutual dependence. If one might imagine the world in such a Utopian condition as would allow commerce to diffuse itself without being affected by political events; if the world were raised to a state which would require no part of human industry to be appropriated to the purposes of governments, nor of its population to be employed in war; mankind, at liberty to cultivate in safety the varieties of trade, would divide themselves into companies, by which an approximation would be induced towards a communion of goods, and society would assume a form of which a faint epitome may sometimes be traced in the communities of factories and colonies;—with this difference, however, that neither the calamities of war nor the struggles of faction interfering to divert the merchant

* Communicated by the Author.

from

from his speculations, the artist from his profession, or the manufacturer from his industry, a more certain and even result might be expected from the infinite sources of commerce. It is evident that the heaviest oppression on trade is occasioned by state necessities: the armies, and the revenue requisite to support wars and the paraphernalia of nations, are obtained at the expense of its capital and the most efficient instruments of labour. But this expense, great as it is directly, is often increased indirectly by those regulations which governments have promulgated for rendering commercial enterprises subservient to political purposes. Experience has proved that commerce furnishes the most powerful engine of war, and statesmen respect trade only for the aids which it yields in war: it is always with a reference to war that it receives the encouragement of statesmen. The view, for instance, with which the British government has so assiduously encouraged the fisheries, had certainly for its object the rearing of seamen for the navy, as much as the local improvement of the districts contiguous to the fishing stations. The navigation act, while it has tended to increase the number of ships and sailors, and thereby promoted the naval superiority of Great Britain, has been perhaps detrimental to the extension of her trade in general by occupying so much capital in the value of ships of war, and so many men in marine labour. But the navigation act being one of those regulations of trade which arise out of political interests, it leads us to consider the second thing proposed, namely, how far such regulations or restrictions ought to extend; in the consideration of which it may be as well to advert to what ought to be the commercial policy of Great Britain.

The political respectability of Great Britain demands, as essential to her preponderancy, the preservation, perhaps the extension, of her naval superiority. Therefore the attention of government should be directed to foster those branches of trade that will employ or increase her shipping, and at the same time encourage her manufactures. To accomplish this, British commerce must be confined to British ships, and all the articles of Britsh trade permitted to pass wherever the merchant conceives a likelihood of obtaining profit. We must even shake off that antiquated prejudice which still makes a distinction between agriculture and trade, and consider agriculture as only one of the many divisions of trade; we must cease to believe that corn more than any other commodity requires a particular system of laws; and if we can import our corn cheaper than it can be

raised at home, we must cease to think that a free importation of corn can be detrimental to our national prosperity. I admit, unquestionably, that a free importation of corn would probably affect the rent-rolls of the landholders; but is there any one who will assert that a reduction of rents produced by such a cause would be a misfortune to the kingdom? If the income of the landholders can only be supported at its present rate by restrictions on the corn trade, it is surely evident that the landholders are benefited at the expense of the nation. But granting that the landholders were injured by removing the restraints of the corn laws, it is highly probable that their misfortune would ultimately become advantageous to themselves; for, to preserve their hereditary importance they might be induced to engage in trade, and with their visible capital would possess an obvious superiority over those who are dependent on industry and enterprise. The fertile tracts of America present the great resource from which the British empire should be supplied with grain. By limiting the supplies which the colonies and the united kingdom might draw from America to the importations of British vessels, the quantity of British shipping would necessarily augment; and the Americans, thus finding an advantage in attending more to agriculture, would resign navigation, and, gradually relinquishing that power upon the seas which they are as gradually obtaining, leave us the mastery which we still possess. It may perhaps be said, that such a restriction as is here proposed might force the Americans to countervail us, by refusing to ship their produce in British vessels. But this is an evil that time would rectify; for we are yet sufficiently independent of American produce to wait patiently the operation of the measure. In the present state of America, it cannot be doubted that agriculture more than commerce ought to engage the inhabitants; so that the very restrictions which would come in to the aid of our advantage, would also be beneficial to them.

There is another point that should be considered here. It ought to be part of the commercial policy of Great Britain to afford every accommodation to the British merchant for trading with foreign colonies, and to prevent foreign merchants from trading with the British colonies. By attending to the former we may obtain the latter: for, as part of our policy is to increase our shipping, we should, by giving occupation to foreigners in their own countries, take away their desire to become navigators by removing the necessity. Thus the nature of British restrictions

tions on trade ought to extend no further than so to press against the commercial systems of other countries as to give them a bias opposite to our own. When more is attempted, the consequences will revert upon ourselves in losses which can neither be calculated nor by any after lenity remedied. But a few facts will more clearly illustrate this truth than any general reasoning, especially if a series can be produced that obviously originated in an act of government. Let us, however, previously consider some of those instances of the folly of partial restrictions on trade, of which the consequences have not been distinctly ascertained, and of which the records are more imperfect.

During a long period the principal trade of Scotland was its fisheries; and the acts of the Scottish legislature, commencing with the reign of the first James of that kingdom, exhibit the unremitted attention which the fisheries received from the government. It would be inconsistent with the limits of an essay to trace here the various effects produced by the different laws which were deemed essential to the improvement of the fisheries: but it is not foreign to the purpose to call the attention of the reader to some of those steps by which the Scottish nation advanced towards that great commercial enterprise the Darien expedition.

From 1424, in which a tax was imprudently levied on the exportation of herrings, till 1493, the Scots appear to have regularly, and with different degrees of success, prosecuted their fisheries. In the year 1493, the naval spirit of that adventurous prince James IV. prompted him to undertake a variety of plans to raise seamen for the navy which he was then building; and, among others, to obtain an act perhaps the most extraordinary in its provisions that ever passed the legislature of any state. Although the 49th cap. of James IV. did not, and indeed could not, produce such a sudden increase of mariners as his impatient genius demanded for its projects, it must be regarded as one of those bold interpositions of authority that generate a successive train of consequences, and become epochs in the history of the affairs to which they relate. By it the boroughs and towns were commanded to build busses and vessels for the fisheries, and to send all idle persons on board. How far this preposterous law was carried into execution is not our present business to examine; it is however well known, that in the reign of James IV., animated by his example and influence, the Scots had reached a high degree of maritime power. We may therefore, without adverting to what may have been done before, presume to

say

say that from this period a foundation was laid for a nursery of seamen in Scotland, and that, though no other record remained, the laws of the subsequent reigns would prove that the fisheries continued to engage the attention of the trading community. Undoubtedly they would be affected by the tumults of the reformation and of the civil wars, but perhaps not to the extent which is generally imagined. For people habitually disposed to industry do not readily enter into political contentions, particularly if their industry be maritime; and the fact is, that both the reformation and the civil wars were managed by the chieftains and their adherents. The boroughs and towns would, no doubt, take an interest in the procession of events during those two tumultuous periods, but not to such a degree as the factious chieftains and their predatory clans were incited. Therefore, when we consider the various laws which were passed in the reign of Charles II. for the encouragement of the Scottish fisheries, we must regard them as directed to their revival, and not their establishment. And when we pass along the intervening events till the year 1694, in which the Darien expedition was projected, we can easily understand that there must have been then in Scotland a number of seamen who would be the first to embark in an enterprise that promised so fairly an unprecedented reward of affluence. The judgment with which that expedition was planned, and the spirit with which it was executed, reflects as much honour upon the Scottish nation as the policy by which it was undermined disgraces the disposition and reign of William III. The English and Dutch East India companies, foreseeing the effects of the advantageous situation which the Scots had selected, influenced William to counteract the expedition; and private partialities, and an ignorance of the true interests of trade, induced him to destroy a colony that by careful fostering would have extended the commerce of his kingdoms. The consequence of William's policy did not terminate in the ruins of the Darien colony; the mariners who sailed with the expedition were for ever lost to the country. The fisheries were for many years after of no importance, and the Dutch in the interval obtained an insurmountable preference for their herrings in the markets of Europe. A reduction in the amount of the fisheries was not the only evil; the proper knowledge of the trade became almost extinguished, and for nearly a century since has the British legislature been annually occupied with schemes and projects to restore that knowledge. It is as well known as any historical truth whatever, that prior to the

the Darien expedition the Scottish fishers practised the deep-sea herring-fishery, which the Dutch have so long so successfully followed; and it is equally well known to every one who has at all attended to the subject, that such has been the total ignorance of that method of fishing among the Scots, that up to the last year not one vessel was fitted out from any port in Scotland for many years before. It is, however, with no small degree of satisfaction, that I have it in my power to be the first to record that several vessels are this year equipped for that purpose from the Clyde. But the diminution of the trade, and the loss of the requisite knowledge, did not close the effects of William's interference with the Darien expedition; the revenue of the kingdom is burthened with heavy bounties, that can only be described as the means of prolonging the precarious loch fishery; and large sums are annually voted to force upon the inhabitants of the Highlands of Scotland that industry and civilization which would probably have gradually arisen from the extensive commerce that would have flowed in upon the mother country from the Darien colony.

Independent of the pecuniary consequences of impolitic restrictions on trade, they are often the cause of political evils also. The American war, and all that has resulted from the consolidation of the United States, will be found to have originated in those severe limitations with which the trade of the colonies was harnessed to forward the prosperity of the mother country, although the separation of the colonies has been ascribed to the financial schemes of the British ministry. The spirit, as well as the letter, of the navigation law was enforced upon the colonies with a degree of rigour altogether obnoxious to the free genius of trade. The colonies were compelled to send their produce to Great Britain in vessels belonging to British subjects, and from Great Britain the rest of the world was supplied with Anglo-American produce. By this restriction the colonies were obliged to furnish themselves from British markets with the necessaries or luxuries that they required, which being of greater value than their produce, in time accumulated a debt against them to so great an amount, that to be released from it formed the reason, and the tax-bills furnished the pretext, for throwing off the yoke of the mother country. Had the British government, instead of expecting to draw from the colonies a direct revenue, made such political arrangements as would have allowed them to trade immediately with the states of Europe, we should not now have been consoling ourselves with the absurd assertion,

tion, that the loss of thirteen provinces was a benefit to the empire. Had the colonies been permitted to trade immediately with the states of Europe, a flow of wealth would have reverted to them that ultimately would have assisted the mother country, who, instead of holding her sovereignty over them by the slender tie of opinion, might have supported, by judicious internal taxation, such a formidable military force in America as would have prevented the division of the empire, and in periods of necessity would have abetted her cause with the greatest effect. But from these general speculations let us turn to the consideration of facts; and, to show that the mercantile machine of our own country is not the only one that has been deranged by the impolitic hands of statesmen, I am induced to quote an instance from the affairs of Russia that is most distinct and unquestionable.

The proprietors of the iron mines of Russia, about the year 1798, took it into their heads that the immense forests of that vast country were diminishing so rapidly, that unless the exportation of timber were prohibited a scarcity of wood must ensue; and they infected the government with the same notion; in consequence of which the exportation of timber was partially prohibited. The British vessels could not, as formerly, obtain deals to make up their cargoes; they were therefore obliged to take a larger quantity of iron, the price of which was raised so high that it could not be sold for an adequate profit in the British markets; and, as the quantity of Russian iron consumed by Great Britain had been annually decreasing for several years before, the prohibition hastened the diminution. In the year 1781 Great Britain alone imported from Petersburgh nearly 50,000 tons of iron; in 1804 the quantity was under 6000. Upon an equality with this measure of the Russian government may be placed that law of ours, which was passed in 1747, to prevent the insurance of French vessels, or their cargoes, in this country during the war with France; in consequence of which regular offices of insurance were established at Paris and in the principal ports of France; and when peace was restored between the two countries, the French had found the way so readily to their own insurance offices, that we never after regained that part of their commercial profit which we formerly received in premiums. But not to spend too much time in quoting detached instances of the folly of limiting the modes and objects of trade, I will endeavour to show that the country gentlemen of England, by a narrow-minded jealousy of the progress which

which the Irish had made in agriculture and farming, towards the close of the 17th century, have abridged, beyond the possibility of remedy, the staple of England, her woollen manufactures. In the reign of Charles II., conceiving the produce of their estates considerably reduced in value by the free importation of cattle and grain from Ireland, they succeeded in setting aside the general interest of the country by obtaining a law to prohibit the importation of such commodities. The Irish, being thus prevented from importing into England, were obliged to salt what they could not find consumption for at home, and export it to other countries for a market; by which that great branch of Irish trade, the exportation of salted provisions, was established. It was long, however, before a sufficient foreign consumption could be obtained, so that their sheep were allowed to increase for the sake of wool alone; by which that commodity was rendered much cheaper in Ireland than in England. The cheapness of wool in Ireland enabled the Irish to set up woollen manufactures of their own, which soon rivalled those of England; so that the English merchants, finding themselves equalled by the Irish, brought down another misfortune upon the general interests of the country. In 1699 an act was passed to prohibit the exportation of woollen manufactures from Ireland to any place except to England and Wales; even to England and Wales the exportation was so unmercifully restricted that this was an indulgence in words only: the consequence was, that many of the Irish manufacturers were obliged to seek employment in foreign countries; and the greater number went to France: by them the woollen manufactures of that kingdom were established, and by their connection a clandestine exportation of wool from Ireland was carried on; so that the French soon made sufficient cloth for themselves, and became our rivals in foreign markets.

It is therefore evident, both from the nature of the thing, and the variety and number of instances which might be given, that freedom is not more essential to commerce than immediate legislative interference is pernicious. Mankind are always regulated in their undertakings by the character of existing circumstances; and the objects of trade vary with political occurrences, which, generally originating from causes the most obscure, are placed beyond the control of preconcerted schemes, and frequently exhibit the reverse of their prospective estimate. Hence it is that laws founded on particular incidents, and intended to promote temporary purposes

purposes or private emolument, must in their operation produce results different from what are previously supposed, and will become the seeds of events of incalculable extent and influence.

XXIII. *Muriatic Solution of Tin in part decomposed into metallic crystallized Tin.* By M. Bucholz*.

It is some time ago since, in the view of preparing the muriate of tin, I treated seven pounds of the finest English tin with fifteen pounds † of muriatic acid weighing 1·120. At the approach of night there still remained from 2 to 2¼ pounds of tin undissolved. The next day the matter was yet luke-warm, and the liquid had the consistency of syrup. I poured gently above it a pound of water ‡, which swam upon the solution. At the end of an hour, while I examined the mixture, I observed with astonishment that the undissolved tin, and particularly its running particles, were covered with a quantity of lances, needles, blades, &c. of metallic crystallized tin, in length from a quarter of an inch to half an inch.

M. Bucholz, having some time after repeated the operation, had the satisfaction to see the same appearance again produced. He assured himself, by all possible trials, that the tin he had employed was absolutely pure. The author proposes different explanations of this phænomenon, which he believes always will be found in opposition to the rules of the dynamic doctrine of Kant on the perfect equilibrium of all parts of a composition, or the perfect equilibrium of principles in the composition. This theory is, however, every moment in contradiction with experience, inasmuch as it does not take into consideration the determinative force of a decided composition, of which the authors have understood as little as of chemistry in general.

We shall not report the different explanations of M. Bucholz, none of which has appeared to us satisfactory.

* From *Van Mons's Journal*, vol. vi.

† In a former notice the author says sixteen.

‡ In the former annonce it is called two pounds.

XXIV. *On*

XXIV. *On muscular Motion. By* Anthony Carlisle, *Esq. F.R.S.: being the Croonian Lecture. Read before the Royal Society November* 8, 1804.

Animal physiology has derived several illustrations and additions from the institution of this lecture on muscular motion, and the details of anatomical knowledge have been considerably augmented by descriptions of muscular parts before unknown.

Still, however, many of the phænomena of muscles remain unexplained, nor is it to be expected that any sudden insulated discovery shall solve such a variety of complicated appearances.

Muscular motion is the first sensible operation of animal life: the various combinations of it sustain and carry on the multiplied functions of the largest animals: the temporary cessation of this motive faculty is the suspension of the living powers, its total quiescence is death.

By the continuance of patient, well-directed researches, it is reasonable to expect much important evidence on this subject; and, from the improved state of collateral branches of knowledge, together with the addition of new sources and methods of investigation, it may not be unreasonable to hope for an ultimate solution of these phænomena, no less complete and consistent than that of any other desideratum in physical science.

The present attempt to forward such designs is limited to circumstances which are connected with muscular motion, considered as causes, or rather as a series of events, all of which contribute, more or less, as conveniencies or essential requisites to the phænomena; the details of muscular applications being distinct from the objects of this lecture.

No satisfactory explanation has yet been given of the state or changes which obtain in muscles during their contractions or relaxations; neither are their corresponding connections with the vascular, respiratory, and nervous systems sufficiently traced. These subjects are therefore open for the present inquiry; and although I may totally fail in this attempt to elucidate any one of the subjects proposed, nevertheless I shall not esteem my labour useless, or the time of the Royal Society altogether unprofitably consumed, if I succeed in pointing out the way to the future attainment of knowledge so deeply interesting to mankind.

The muscular parts of animals are most frequently composed

posed of many substances, in addition to those which are purely muscular. In this gross state they constitute a flexible, compressible solid, whose texture is generally fibrous, the fibres being compacted into fasciculi, or bundles, of various thickness. These fibres are elastic during the contracted state of muscles after death, being capable of extension to more than one-fifth of their length, and of returning again to their former state of contraction.

This elasticity, however, appears to belong to the enveloping reticular or cellular membrane, and it may be safely assumed that the intrinsic matter of muscle is not elastic.

The attraction of cohesion, in the parts of muscle, is strongest in the direction of the fibres, it being double that of the contrary, or transverse, direction.

When muscles are capable of reiterated contractions and relaxations, they are said to be alive, or to possess irritability. This quality fits the organ for its functions. Irritability will be considered, throughout the present lecture, as a quality only.

When muscles have ceased to be irritable, their cohesive attraction in the direction of their fibres is diminished, but it remains unaltered in the transverse direction.

The hinder limbs of a frog attached to the pelvis being stripped of the skin, one of them was immersed in water, at 115° of Fahrenheit, during two minutes; when it ceased to be irritable. The thigh bones were broken in the middle, without injuring the muscles, and a scale affixed to the ancle of each limb: a tape passed between the thighs was employed to suspend the apparatus. Weights were gradually introduced into each scale, until, with five pounds avoirdupois, the dead thigh was ruptured across the fleshy bellies of its muscles.

The irritable thigh sustained six pounds weight avoirdupois, and was ruptured in the same manner. This experiment was repeated on other frogs, where one limb had been killed by a watery solution of opium, and on another where essential oil of cherry laurel* was employed: in each experiment the irritable limb sustained a weight one-sixth heavier than the dead limb.

It may be remarked, in confirmation of these experiments, that when muscles act more powerfully or more rapidly than is equal to the strength of the sustaining parts, they do not usually rupture their fleshy fibres, but break their tendons, or even an intervening bone, as in the in-

* Distilled oil from the leaves of the *prunus lauro-cerasus.*

stances

stances of ruptured tendo achillis, and fractured patella. Instances have, however, occurred, wherein the fleshy bellies of muscles have been lacerated by spasmodic actions; as in tetanus the recti abdominis have been torn asunder, and the gastrocnemii in cramps; but in those examples it seems that either the antagonists produce the effect, or the over-excited parts tear the less excited in the same muscle. From whence it may be inferred, that the attraction of cohesion in the matter of muscle is considerably greater during the act of contracting, than during the passive state of tone, or irritable quiescence; a fact which has been always assumed by anatomists from the determinate forces which muscles exert.

The muscular parts of different classes of animals vary in colour and texture, and not unfrequently those variations occur in the same individual.

The muscles of fishes and vermes are often colourless, those of the mammalia and birds being always red: the amphibia, the accipenser, and squalus genera, have frequently both red and colourless muscles in the same animal.

Some birds, as the black game*, have the external pectoral muscles of a deep red colour, whilst the internal are pale.

In texture, the fasciculi vary in thickness; and the reticular membrane is in some parts coarse, and in others delicate: the heart is always compacted together by a delicate reticular membrane, and the external glutæi by a coarser species.

An example of the origin of muscle is presented in the history of the incubated egg; but whether the rudiments of the punctum saliens be part of the cicatricula organized by the parent, or a structure resulting from the first process of incubation, may be doubtful: the little evidence to be obtained on this point seems in favour of the former opinion; a regular confirmation of which would improve the knowledge of animal generation by showing that it is gemmiferous. There are sufficient analogies of this kind in nature, if reasoning from analogies were proper for the present occasion.

The punctum saliens, during its first actions, is not encompassed by any fibres discoverable with microscopes, and the vascular system is not then evolved, the blood flowing forwards and backwards, in the same vessels. The commencement of life in animals of complex structure is,

* *Tetrao tetrix.* Linn.

from the preceding fact, like the ultimate organization of the simpler classes.

It is obvious that the muscles of birds are formed out of the albumen ovi, the vitellus, and the atmospheric air, acted upon by a certain temperature. The albumen of a bird's egg is wholly consumed during incubation, and the vitellus little diminished, proving that the albumen contains the principal elementary materials of the animal thus generated; and it follows that the muscular parts, which constitute the greater proportion of such animals when hatched, are made out of the albumen, a small portion of the vitellus, and certain elements, or small quantities of the whole compound of the atmosphere.

The muscles of birds are not different, in any respect, from those of quadrupeds of the class of mammalia.

The anatomical structure of muscular fibres is generally complex, as those fibres are connected with membrane, blood-vessels, nerves, and lymphæducts; which seem to be only appendages of convenience to the essential matter of muscle.

A muscular fibre, duly prepared by washing away the adhering extraneous substances, and exposed to view in a powerful microscope, is undoubtedly a solid cylinder, the covering of which is reticular membrane, and the contained part a pulpy substance irregularly granulated, and of little cohesive power when dead.

A difficulty has often subsisted among anatomists, concerning the ultimate fibres of muscles; and, because of their tenuity, some persons have considered them infinitely divisible; a position which may be contradicted at any time by an hour's labour at the microscope.

The arteries arboresce copiously upon the reticular coat of the muscular fibre, and in warm blooded animals these vessels are of sufficient capacity to admit the red particles of blood; but the intrinsic matter of muscle, contained within the ultimate cylinder, has no red particles.

The arteries of muscles anastomose with corresponding veins; but this course of a continuous canal cannot be supposed to act in a direct manner upon the matter of muscle.

The capillary arteries terminating in the muscular fibre must alone effect all the changes of increase in the bulk or number of fibres, in the replenishment of exhausted materials, and in the repair of injuries: some of these necessities may be supposed to be continually operating. It is well known that the circulation of the blood is not essential to muscular action; so that the mode of distribution of the blood-

blood-vessels, and the differences in their size, or number, as applied to muscles, can only be adaptations to some special convenience.

Another prevalent opinion among anatomists is, the infinite extension of vascularity, which is contradicted in a direct manner by comparative researches. The several parts of a quadruped are sensibly more or less vascular, and of different contextures: and admitting that the varied diameter of the blood-vessels disposed in each species of substance, were to be constituted by the gross sensible differences of their larger vessels only; yet, if the ultimate vessels were in all cases equally numerous, then the sole remaining cause of dissimilarity would be in the compacting of the vessels. The vasa vasorum of the larger trunks furnish no reason, excepting that of a loose analogy, for the supposition of vasa vasorum extended without limits. Moreover, as the circulating fluids of all animals are composed of water, which gives them fluidity, and of animalized particles of defined configuration and bulk; it follows, that the vessels through which such fluids are to pass must be of sufficient capacity for the size of the particles, and that smaller vessels could only filtrate water devoid of such animal particles: a position repugnant to all the known facts of the circulation of blood, and the animal œconomy.

The capillary arteries, which terminate in the muscular fibre, must be secretory vessels for depositing the muscular matter, the lymphæducts serving to remove the superfluous extravasated watery fluids, and the decayed substances which are unfit for use.

The lymphæducts are not so numerous as the blood-vessels, and certainly do not extend to every muscular fibre: they appear to receive their contained fluids from the intersticial spaces formed by the reticular or cellular membrane, and not from the projecting open ends of tubes, as is generally represented. This mode of receiving fluids out of a cellular structure, and conveying them into cylindrical vessels, is exemplified in the corpora cavernosa and corpus spongiosum penis, where arterial blood is poured into cellular or reticular cavities, and from thence it passes into common veins by the gradual coarctation of the cellular canals.

In the common green turtle, the lacteal vessels universally arise from the loose cellular membrane, situated between the internal spongy coat of the intestines and the muscular coat. The cellular structure may be filled from the lacteals, or the lacteals from the cellular cavities. When

injecting the smaller branches of the lymphæducts retrograde in an œdematous human leg, I saw, very distinctly, three orifices of these vessels terminating in the angles of the cells, into which the quicksilver trickled. The preparation is preserved, and a drawing of the appearance made at the time. It was also proved, by many experiments, that neither the lymphæducts nor the veins have any valves in their minute branches.

The nerves of voluntary muscles separate from the same bundles of fibrils with the nerves which are distributed in the skin, and other parts, for sensation; but a greater proportion of nerve is appropriated to the voluntary muscles than to any other substances, the organs of the senses excepted.

The nerves of volition all arise from the parts formed by the junction of the two great masses of the brain, called the *cerebrum* and *cerebellum*, and from the extension of that substance throughout the canal of the vertebræ. Another class of muscles, which are not subject to the will, are supplied by peculiar nerves; they are much smaller, in proportion to the bulk of the parts on which they are distributed, than those of the voluntary muscles; they contain less of the white opake medullary substance than the other nerves, and unite their fibrils, forming numerous anastomoses with all the other nerves of the body, excepting those appropriated to the organs of the senses. There are enlargements at several of these junctions, called *ganglions*, and which are composed of a less proportion of the medullary substance, and their texture is firmer than that of ordinary nerves.

The terminal extremities of nerves have been usually considered of unlimited extension: by accurate dissection, however, and the aid of magnifying-glasses, the extreme fibrils of nerves are easily traced as far as their sensible properties, and their continuity extend. The fibrils cease to be subdivided, whilst perfectly visible to the naked eye, in the voluntary muscles of large animals; and the spaces they occupy upon superficies where they seem to end, leave a remarkable excess of parts unoccupied by those fibrils. The extreme fibrils of nerves lose their opacity, the medullary substance appears soft and transparent, the enveloping membrane becomes pellucid, and the whole fibril is destitute of the tenacity necessary to preserve its own distinctness; it seems to be diffused and mingled with the substances in which it ends. Thus the ultimate terminations of nerves for volition, and ordinary sensation, appear to be, in

in the reticular membrane, the common covering of all the different substances in an animal body, and the connecting medium of all dissimilar parts.

By this simple disposition, the medullary substance of nerve is spread through all organized, sensible, or motive parts, forming a continuity which is probably the occasion of sympathy. Peculiar nerves, such as the first and second pairs, and the portio mollis of the seventh, terminate in an expanse of medullary substance which combines with other parts and membranes, still keeping the sensible excess of the peculiar medullary matter.

The peculiar substance of nerves must in time become inefficient; and, as it is liable to injuries, the powers of restoration, and repair are extended to that material. The reunion of nerves after their division, and the reproduction after part of a nerve has been cut away, have been established by decisive experiments. Whether there is any new medullary substance employed to fill up the break; and, if so, whether the new substance be generated at the part, or protruded along the nervous theca from the brain, are points undetermined: the history of the formation of a fœtus, the structure of certain monsters, and the organization of simple animals, all seem to favour the probability that the medullary matter of nerves is formed at the parts where it is required, and not in the principal seat of the cerebral medulla.

This doctrine, clearly established, would lead to the belief of a very extended commixture of this peculiar matter in all the sensible and irritable parts of animals, leaving the nerves, in their limited distribution, the simple office of conveying impressions from the two sentient masses with which their extremities are connected. The most simple animals, in whom no visible appearances of brain or nerves are to be found, and no fibrous arrangement of muscles, may be considered of this description: Mr. John Hunter appeared to have had some incomplete notions upon this subject, which may be gathered from his representation of a materia vitæ in his Treatise on the Blood, &c. Perhaps it would be more proper to distinguish the peculiar matter of muscle by some specific term, such, for example, as materia contractilis.

A particular adaptation for the nerves which supply the electrical batteries of the torpedo and and gymnotus, is observable on the exit of each from the skull; over which there is a firm cartilage acting as a yoke, with a muscle affixed to it, for the obvious purpose of compression: so

that a voluntary muscle probably governs the operations of the battery.

The matter of the nerves and brain is very similar in all the different classes of animals.

The external configuration of animals is not more varied than their internal structure.

The bulk of an animal, the limitation of its existence, the medium in which it lives, and the habits it is destined to pursue, are each, and all of them, so many indications of the complexity or simplicity of their internal structure. It is notorious that the number of organs, and of members, is varied in all the different classes of animals: the vascular and nervous systems, the respiratory and digestive organs, the parts for procreation, and the instruments of motion, are severally varied, and adapted to the condition of the species. This modification of anatomical structure is extended in the lowest tribes of animals until the body appears to be one homogeneous substance. The cavity for receiving the food is indifferently the internal or external surface; for they may be inverted, and still continue to digest food: the limbs, or tentacula, may be cut off, and they will be regenerated without apparent inconvenience to the individual: the whole animal is equally sensible, equally irritable, equally alive: its procreation is gemmiferous. Every part is pervaded by the nutritious juices, every part is acted upon by the respiratory influence, every part is equally capable of motion, and of altering its figure in all directions, whilst neither blood-vessels, nerves, nor muscular fibres are discoverable by any of the modes of investigation hitherto instituted.

From this abstract animal (if such a term may be admitted) up to the human frame, the variety of accessory parts, and of organs by which a complicated machinery is operated, exhibit infinite marks of design, and of accommodations to the purposes which fix the order of nature.

In the more complicated animals there are parts adapted for trivial conveniences, much of their materials not being alive, and the entire offices of some liable to be dispensed with. The water transfused throughout the intersticial spaces of the animal fabric; the combinations with lime in bones, shells, and teeth; the horns, hoofs, spines, hairs, feathers, and cuticular coverings, are all of them, or the principal parts of their substance, extra-vascular, insensible, and unalterable by the animal functions after they are completed. I have formed an opinion, grounded on extensive observation, that many more parts of animal bodies may

may be considered as inanimate substances; even the reticular membrane itself seems to be of this class, and tendons, which may be the condensed state of it: but these particulars are foreign to the present occasion.

The deduction now to be made, and applied to the history of muscular motion, is, that animated matter may be connected with inanimate: this is exemplified in the adhesions of the muscles of multivalve and bivalve shellfish to the inorganic shell, the cancer Bernhardus to the dead shells of other animals, and in the transplantation of teeth. All of which, although somewhat contrary to received opinion, have certainly no degree of vascularity, or vital connection with the inhabitant; these shells being liable to transudations of cupreous salts and other poisonous substances, whilst the animal remains uninjured. A variety of proofs to the same effect might be adduced, but it would be disrespectful to this learned body to urge any further illustrations on a subject so obvious.

The effects of subdivision, or comminution of parts among the complicated organized bodies, is unlike that of mineral bodies: in the latter instance, the entire properties of the substance are retained, however extensive the subdivision; in the former substances, the comminution of parts destroys the essential texture and composition, by separating the gross arrangements of structure upon which their specific properties depend. From similar causes it seems to arise, that animals of minute bulk are necessarily of simple structure: size alone is not, however, the sole cause of their simple organization, because examples are sufficiently numerous wherein the animal attains considerable bulk, and is of simple structure, and *vice versâ*; but, in the former, the medium in which they live, and the habits they assume, are such as do not require extensive appendages, whilst the smaller complex animals are destined to more difficult and more active exertions. It may be assumed, however, as an invariable position, that the minutest animals are all of simple organization.

Upon a small scale, life may be carried on with simple materials; but the management and provisions for bulky animals, with numerous limbs, and variety of organs, and appendages of convenience, are not effected by simple apparatus: thus the skeleton which gives a determinate figure to the species, supports its soft parts, and admits of a geometrical motion, is placed interiorly, where the bulk of the animal admits of the bones being sufficiently strong, and yet light enough for the moving powers; but the skeleton is

is placed externally, where the body is reduced below a certain magnitude, or where the movements of the animal are not to be of the floating kind; in which last case the bulk is not an absolute cause. The examples of testaceous vermes, and coleopterous, as well as most other insects, are universally known.

The opinion of the muscularity of the crystalline lens of the eye, so ingeniously urged by a learned member of this society, is probably well founded; as the arrangement of radiating lines of the matter of muscle, from the centre to the circumference of the lens, and these compacted into angular masses, would produce specific alterations in its figure.

This rapid sketch of the history of muscular structure has been obtruded before the Royal Society to introduce the principal experiments and reasonings which are to follow: they are not ordered with so much exactness as becomes a more deliberate essay; but the intention already stated, and the limits of a lecture, are offered as the apology.

Temperature has an essential influence over the actions of muscles: but it is not necessary that the same temperature should subsist in all muscles during their actions; neither is it essential that all the muscular parts of the same animal should be of uniform temperatures for the due performance of the motive functions.

It appears that all the classes of animals are endowed with some power of producing thermometrical heat, since it has been so established in the amphibia, pisces, vermes, and insecta, by Mr. John Hunter; a fact which has been verified to my own experience: the term *cold-blooded* is therefore only relative. The ratio of this power is not, however, in these examples, sufficient to preserve their equable temperature in cold climates; so that they yield to the changes of the atmosphere, or the medium in which they reside, and most of them become torpid, approaching to the degree of freezing water. Even the mammalia and aves possess only a power of resisting certain limited degrees of cold; and their surfaces, as well as their limbs, being distant from the heart and principal blood-vessels, the muscular parts so situated are subject to considerable variations in their temperature, the influence of which is known.

In those classes of animals which have little power of generating heat, there are remarkable differences in the structure of their lungs, and in the composition of their blood, from the mammalia and aves.

Respiration is one of the known causes which influences the

the temperatures of animals: where these organs are extensive, the respirations are performed at regular intervals, and are not governed by the will, the whole mass of blood being exposed to the atmosphere in each circulation. In all such animals living without the tropics, their temperature ranges above the ordinary heat of the atmosphere, their blood contains more of the red particles than in the other classes, and their muscular irritability ceases more rapidly after violent death.

The respirations of the animals denominated cold-blooded are effected differently from those of high temperature; in some of them, as the amphibia of Linnæus, the lungs receive atmospheric air, which is arbitrarily retained in large cells, and not alternately and frequently changed. The fishes, and the testaceous vermes, have lungs which expose their blood to water; but whether the water alone, or the atmospheric air mingled with it, furnish the changes in the pulmonary blood, is not known.

In most of the genera of insects the lungs are arborescent tubes containing air, which, by these channels, is carried to every vascular part of the body. Some of the vermes of the simpler construction have no appearance of distinct organs; but the respiratory influence is nevertheless essential to their existence, and it seems to be effected on the surface of the whole body.

In all the colder animals the blood contains a smaller proportion of the red colouring particles than in the mammalia and aves: the red blood is limited to certain portions of the body, and many animals have none of the red particles.

[To be continued.]

XXV. *Account of Mr.* Arthur Woolf's *new Improvements on Steam-Engines.*

In our nineteenth volume, p. 133, we gave a short account of a former improvement made by Mr. Woolf on the steam-engine, founded on a discovery that steam, of any higher temperature than that of boiling water, if allowed to pass into another vessel kept at the same temperature as the steam itself, will expand to as many times its volume, and still be equal to the pressure of the common atmosphere, as the number of pounds which such steam, before being allowed to expand, could maintain on each square inch of a safety-

safety-valve exposed to the atmosphere: for example, that masses or quantities of steam of the expansive force of 20, 30, or 50 pounds the square inch of a common safety-valve, will expand to 20, 30, or 50 times its volume, and still be respectively equal to the atmosphere, or capable of producing a sufficient action against the piston of a steam-engine to cause the same to rise in the old engine (with a counterpoise) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines first brought into effect by Messrs. Boulton and Watt.

In consequence of this discovery Mr. Woolf was enabled to use his steam twice (if he chose), and with complete effect; nothing more being necessary than to admit high steam, suppose of 40 pounds the square inch, into one cylinder, to work there by its expansive force, and then to allow the same steam to pass into, and expand itself in, another cylinder of forty times the size of the first, there to work by condensation in the common way. Or with only one cylinder, by admitting a proportionally small quantity of high steam into it from the boiler, Mr. Woolf found that he could effect a considerable saving in fuel.

In this first improvement of Mr. Woolf, though the saving might be carried a considerable length, it was still necessarily limited by the strength of materials; for in the employment of high steam there must always be some danger of an explosion. Mr. Woolf, however, by a happy thought, has completely obviated every danger of this kind, and can now take the full advantage of the expansive principle without the least danger whatever. This he effects by throwing into common steam the additional temperature necessary for its high expansion, *after the steam is admitted into the working cylinder*, which is heated by means adequate to the end intended to be obtained; and the advantage which he thus gains he effectually secures by a most ingenious improvement in the piston. It may be easily conceived that steam of such high rarity as Mr. Woolf employs, could not be made fully effective with the piston in common use; for in proportion to its rarity so must be the facility with which a portion of it would escape, and pass by the side of the piston to the vacuous part of the cylinder: but Mr. Woolf's contrivance seems perfectly adapted to prevent the loss of even the smallest portion of the steam.

Besides these improvements on the common steam-engine, he has also found means to apply the same principles to the old engine, known by the name of Savary's, in such

a way

a way as to render the same a powerful and œconomical engine for a great variety of purposes.

Such is the outline of Mr. Woolf's improvements on this most useful engine: but, for the general information of practical engineers, we shall here subjoin a more technical description, in Mr. Woolf's own words, extracted from his specification of his patent.

"I have found out and invented a contrivance, by which the temperature of the steam vessel or working cylinder of a steam-engine, or of the steam vessels or cylinders where more than one are used, may be raised to any required temperature, without admitting steam from the boiler into any surrounding receptacle, whether known by the name of a steam case, or by any other denomination. That is to say, instead of admitting steam of a high temperature into such receptacle or steam case, which is always attended with a risk of explosion proportioned to the elasticity of the steam employed, I put into the said surrounding receptacle, or case, oil or the fat of animals, or wax, or other substances capable of being melted by a lower temperature than the heat intended to be employed, and of bearing that heat without being converted into vapour: or I put into the said case or cases mercury or mixtures of metals, as of tin, bismuth, and lead, capable of being kept in a state of fusion in a lower temperature than that intended to be employed in working the steam-engine; and I so form the surrounding case or cases as to make it or them admit the aforesaid oil, or other substance employed, to come into contact not only with the sides of the steam vessel or vessels, or working cylinder or cylinders, but also with the bottom and top of the same, so that the whole may be as much as possible maintained at one uniform temperature; and this temperature I keep up by a fire immediately under or round the case or cases that contains the aforesaid oil or other substance, or by connecting the said case or cases with a separate vessel or vessels, kept at a proper temperature, filled with the oil or other substance made use of as aforesaid. In some circumstances, or whenever the same may be convenient or desirable, I employ the fluid metals, or mixtures of metals, and oil or other of the substances before enumerated, at one and the same time in the same engine: that is to say, in the part of the case or vessel exposed to the greatest action of the fire, I sometimes have the aforesaid metals or mixtures of metals, and in the parts less exposed to the action of the fire, I put oil, or other substances capable

pable of bearing the requisite heat without being converted into vapour.

"By this arrangement, and method of applying the surrounding heat, I not only obviate the necessity of employing steam of a great expansive force round the steam vessel or vessels, or the working cylinder or cylinders, as already mentioned, to maintain them at the temperature required, but I am enabled to obtain from steam of a comparatively low temperature, or even from water itself admitted into the steam vessel or vessels, all the effects that can be obtained from steam of a high temperature, without any of the risk with which the production of the latter is accompanied, not only to the boiler and other parts of the machinery, but even to the lives of the workmen; for such low steam, or even water, (but in every case steam is preferable,) being admitted into a steam vessel or vessels, or working cylinder or cylinders, kept at the requisite higher temperature by the forementioned means, will there be expanded in any ratio required, and produce an effect in the working of the engine which cannot otherwise be obtained but at a greater expense of fuel, or with the risk of an explosion. By this means I can make use of steam expanded in any required ratio, or of any given temperature, without the necessity of ever having the steam of any greater elasticity than equal to the pressure of the common atmosphere.

"Another improvement which I make use of in steam-engines consists in a method of preventing, as much as possible, the passage of any of the steam from that side of the piston which is acted upon by the said steam to the other side which is open to the condenser; and this I effect, in those steam-engines known by the name of double engines, by employing upon or above the piston mercury or fluid metal, or metals in an altitude equal to the pressure of the steam. The efficacy of this arrangement will appear obvious, from attending to what must take place in working such a piston. When the piston is ascending, that is, when the steam is admitted below the piston, the space on its other side being open to the condenser, the steam endeavouring to pass up by the side of the piston is met and effectually prevented by the column of metal equal or superior to it in pressure, and during the down stroke no steam can possibly pass without first forcing all the metal through. In working what is called a single engine, a less considerable altitude of metal is required, because the steam always

always acts on the upper side of the piston. For single engines, oil, or wax, or fat of animals, or similar substances, in sufficient quantity, will answer the purpose, if another improvement, which constitutes part of my said invention, be applied to the engine, namely, to take care that in either the double or single engine so to be worked, the outlet that conveys the steam to the condenser shall be so posited, and of such a size, that the steam may pass without forcing before it or carrying with it any of the metal or other substance employed, that may have passed by the piston; taking care at the same time to provide another exit for the metal or other substance collected at the bottom of the steam vessel or working cylinder to convey the same into a reservoir kept at a proper heat, whence it is to be conveyed to the upper side of the piston by a small pump worked by the engine or by any other contrivance. In order that the fluid metal or metals used with the piston may not be oxidated, I always keep some oil or other fluid substance on its surface, to prevent its coming in contact with the atmosphere; and to prevent the necessity of employing a large quantity of fluid metal, I generally make my piston of the depth of the column required, but of a diameter a little less than the steam vessel or working cylinder, excepting where the packing or other fitting is necessary to be applied; so that, in fact, the column of fluid metal forms only a thin body round the piston. In some cases I make a hollow metallic piston, and apply an altitude of fluid metal in the inside of the same, to press its outside into contact with the steam vessel or working cylinder.

"It may be necessary, however, to state, that in applying my improved method of keeping the steam vessels of steam-engines at any required temperature to the engine known by the name of Savary's, in any of its improved forms, in which a separate condenser has been introduced, I sometimes employ oil (or any other substance lighter than water, and capable of being kept fluid in the temperature employed, without being converted into vapour,) in the upper part of the tube or pipe attached to the steam vessel; by which means steam of any temperature may be used without being exposed to the risk of partial condensation by the admission of any colder body into the steam vessel; for the oil, or other substance employed for this purpose, soon acquires the requisite temperature; and to prevent unnecessary escape of heat, I construct of, or line with, an imperfect conductor of heat, that part of the tube or pipe attached to the steam vessel which may not be heated exteriorly.

teriorly. And further, (as is already the practice in some engines, and therefore not exclusively claimed by me,) I cause the water raised by the engine to pass off through another ascending tube than the one attached to the steam vessel, but connected with it at some part lower than the oil or other substance employed in it is ever suffered to descend to in the working of the engine. The improvement which I have just mentioned, of introducing oil into the pipe attached to the steam vessel of such engines, may also be introduced without applying heat externally to the steam vessel; but in this case part of the effect which would otherwise be gained is lost."

XXVI. *Extract of a Letter from* M. Rinck, *of Treysa, on a new Acid found in Alkaline Prussiates* *.

You are undoubtedly acquainted with the Prolusions of Winterl, and you will have verified the most part of his arguments. I should wish to know how you have judged of this work. In Germany, this author and his book were at first exceedingly ill used. They rejected indiscriminately, and without examination, all that M. Winterl had advanced. But of late his opinions have been discussed, and people begin to do him justice. His system has now many supporters, and deserves, by all accounts, considerable attention.

Among the observations of Winterl, we find the following: that in the prussiate and carbonate of potash there exists a salt which dissolves in alcohol, does not precipitate the solution of iron in a blue but in a red colour, and is essentially distinguished from prussiate of potash by its other properties. Although the Prussian lixivium has been carefully examined by many chemists, none of them has recognised this salt. However, it is found in it; and I myself have obtained it. I am occupied at this moment in a train of experiments on this subject, as well as on the formation of prussic acid, which have already afforded some interesting results, and which promise to furnish me with some new facts.

Some carbonated iron which I examined offered me some particular phænomena. Weak acids exercise hardly any action, when cold, upon this iron. They require for this effect the aid of heat, or to be concentrated. The nitric

* From *Van Mons's Journal*, vol. vi.

acid

acid was decomposed, and nitrous gas was disengaged. From the sulphuric and muriatic acids hydrogen were evolved. This fossil blackens at a red heat. The carbonic acid was decomposed, and disengaged itself under the form of gaseous oxide of carbon; and the iron became still more oxidated. This metal is found in the carbonate at the minimum of oxidation, and under the form of white oxide*.

It would be difficult, considering the concomitant disengagement of the other gases, to determine justly what is contained in the carbonic gas of this carbonated iron.

We know that Morecchini has found fluoric acid in fossil elephants' bones. Does not this fact, which is confirmed by Klaproth, lead us to suspect that phosphoric acid transforms itself into fluoric acid? Many appearances come in aid of this suspicion, and, among others, the property of phosphoric acid to corrode glass: the common property of fluates and phosphates of lime to yield a pyrophoric light, &c. The observations made by Gehlen on the occasion of his trials upon æther with fluoric acid, deserve on this account an equal attention. And Klaproth, in analysing the topaz, in which he found 0·05 – 0·07 parts of fluoric acid, has observed another fact, which is particularly remarkable, viz. that the loss which this fossil undergoes by ignition is always more considerable in crucibles of lead than of clay; although, after the observations of Guyton on the bad quality of carbons for conducting heat, one would have expected a contrary effect, which appears to indicate that in the former crucibles the topaz undergoes another action than that of fire.

XXVII. *Chemico-Galvanic Observations. By M.* Oersted †.

I have lately discovered a Galvanic phænomenon hitherto unknown. I was led to it by an experiment of M. Ritter, published some years ago. This philosopher had found that

* There is in this department, in the neighbourhood of Jodogne, a mine of vitriol of iron in great crystalline masses perfectly transparent and colourless, which, by its appearance, one would take for Glauber's salts. The earth which surrounds this salt consists, in that part of it which is not in contact with the air, almost entirely of black oxide of iron. The salt itself, when it is dissolved, becomes green by its being exposed to the air, after which it is precipitated by the same alkalis under the ordinary colour. My colleague De Roover, who has made me acquainted with this salt, has received solid masses of it many pounds in weight.—V. M.

† From *Van Mons's Journal*, vol. vi.

the conducting wires of a pile held in the flame of a taper were covered with figures of soot, which on the negative wire took the form of a vegetation, and on the positive wire a different form. It was to be supposed from this that all Galvanic oxidation was accompanied with a similar phænomenon. To assure myself of this, I put the two poles of the pile in communication with two solutions of the acetate of lead. On the positive side the oxide of lead was precipitated in a brown colour: on the negative side it was reduced. The oxide took a form similar to the figures of soot of the positive pole; and the reduced metal formed a beautiful vegetation. The form of the oxide was like the roots of plants.

In the course of his experiments on the commotion which the mercury experienced, Ritter had remarked that this metal became less liquid on the positive side, and more liquid on the negative side. I have repeated this experiment, and found it confirmed: however, to assure myself still more completely of the existence of the effect, I made the experiment with an amalgamation of lead, which I kept melted under hot water. I let the water cool in communication with the poles of the pile, and I remarked that towards the positive pole the amalgamation was consolidated more quickly than towards the negative. This fact, moreover, agrees with two other phænomena observed by Ritter, viz. that the spark of the positive side inflames the leaves of metals, whilst that of the negative side melts them; and that the hydrogen pole excites a sensation of heat, whilst the positive pole does not, but rather excites a sensation of cold.

XXVIII. *On the Art of Aquatinta Engraving; with a Description of an Apparatus to prevent the Inconvenience which Artists experience from the Fumes which are produced by the Action of the Acid employed in the Process.*

THE principal intention of the present article is to describe the apparatus mentioned in the title. Some of our readers may, however, be gratified by finding along with it some account of the art in which the contrivance is proposed to be used. We therefore insert here the best description we have yet met with respecting that branch of engraving, extracted from the new edition of Dr. Rees's Cyclopædia.

The art of aquatinta was invented by Le Prince, a French artist,

artist, who for a long time kept his process a secret. It has, however, been much improved since. It consists in corroding a copper-plate with aquafortis in such a manner that an impression from it has an appearance very much resémbling a drawing in Indian ink. "This is effected by covering the copper with a powder or some substance which takes a granulated form, so as to prevent the aquafortis from acting where the particles adhere, and by this means cause it to corrode the copper partially and in the interstices only. When these particles are extremely minute, and near to each other, the impression from the plate appears to the naked eye exactly like a wash of Indian ink. But when they are larger, the granulation is more distinct; and as this may be varied at pleasure, it is capable of being adapted with great success to a variety of purposes and subjects.

"This powder or granulation is called the aquatinta grain, and there are two general modes of producing it.

"We shall first describe what is called the powder grain, because it was the first that was used. Having etched the outline on a copper-plate prepared in the usual way by the coppersmith, (for which see the article *Etching*,) some substance must be finely powdered and sifted which will melt with heat, and when cold adhere to the plate, and resist the action of aquafortis. The substances which have been used for this purpose, either separately or mixed, are asphaltum, Burgundy pitch, resin, gum-copal, and gum-mastic; and, in a greater or less degree, all the resins and gum-resins will answer the purpose. Common resin has been most generally used, and answers tolerably well; though gum-copal makes a grain that resists the aquafortis better. The substance intended to be used for the grain must now be distributed over the plate as equally as possible; and different methods of performing this essential part of the operation have been used by different engravers, and at different times. The most usual way is to tie up some of the powder in a piece of muslin, and to strike it against a piece of stick held at a considerable height above the plate. By this, the powder that issues falls gently, and settles equally over the plate. Every one must have observed how uniformly hair-powder settles upon the furniture after the operations of the hair-dresser: this may afford a hint towards the best mode of performing this part of the process. The powder must fall upon it from a considerable height, and there must be a sufficiently large cloud of dust formed. The plate, being covered equally over with the dust or powder, the operator is next to proceed to fix it upon the plate,

by heating it gently, so as to melt the particles. This may be effected by holding under the plate lighted pieces of brown paper rolled up, and moving them about till every part of the powder is melted. This will be known by its change of colour, which will turn brownish. It must now be suffered to cool, when it may be examined with a magnifier; and if the grains or particles appear to be uniformly distributed it is ready for the next part of the process.

"The design or drawing to be engraved must now be examined, and such parts of it as are perfectly white are to be remarked. Those corresponding parts of the plate must be covered, or stopped out, as it is called, with turpentine, or what is better, mastic varnish, diluted with turpentine to a proper consistence to work freely with the pencil, and mixed with lamp-black to give it colour; for, if transparent, the touches of the pencil would not be so distinctly seen. The margin of the plate must also be covered with varnish. When the stopping out is sufficiently dry, a border of wax must be raised round the plate in the same manner as in etching, and the aquafortis, properly diluted with water, poured on. This is called biting in; and it is that part of the process which is most uncertain, and which requires the greatest degree of experience. When the aquafortis has lain on so long that the plate, when printed, would produce the lightest tint in the drawing, it is poured off, and the plate washed with water, and dried. When it is quite dry, the lightest tints are stopped out, and the aquafortis poured on as before; and this is repeated as often as there are tints to be produced in the plate.

"Although many plates are etched entirely by this method of stopping out and biting in alternately, yet it may be easily conceived that in general it would be very difficult to stop round and leave out all the finishing touches, as also the leaves of trees, and many other objects, which it would be impossible to execute with the necessary degree of freedom in this manner.

"To overcome this difficulty, another very ingenious process has been invented, by which the touches are laid on the plate with the same ease and expedition as they are in a drawing in Indian ink. Fine washed whiting is mixed with a little treacle or sugar, and diluted with water in the pencil, so as to work freely, and this is laid on the plate covered with the aquatint ground, in the same manner and on the same parts as ink on the drawing. When this is dry, the whole plate is varnished over with a weak and thin varnish of turpentine, asphaltum, or mastic, and then suffered

fered to dry, when the aquafortis is poured on. The varnish will immediately break up in the parts where the treacle mixture was laid, and expose all those places to the action of the acid, while the rest of the plate remains secure. The effect of this will be, that all the touches, or places where the treacle was used, will be bit in deeper than the rest, and will have all the precision of touches in Indian ink.

"After the plate is completely bit in, the bordering wax is taken off by heating the plate a little with a lighted piece of paper; and it is then cleared from the ground and varnish by oil of turpentine, and wiped clean with a rag and a little fine whiting, and then it is ready for the printer.

"The principal disadvantages of this method of aquatinting are, that it is extremely difficult to produce the required degree of coarseness or fineness in the grain, and that plates so engraved do not print many impressions without wearing out. It is therefore now very seldom used, though it is occasionally of service.

"We next proceed to describe the second method of producing the aquatint ground, which is generally adopted. Some resinous substance is dissolved in spirits of wine, as for instance common resin, Burgundy pitch, or mastic, and this solution is poured all over the plate, which is then held in a slanting direction till all the superfluous fluid drains off, and it is then laid down to dry, which it does in a few minutes. If the plate be then examined with the magnifier, it will be found that the spirit in evaporating has left the resin in a granulated state; or rather, that the latter has cracked in every possible direction, still adhering firmly to the copper. A grain is thus produced with the greatest ease, which is extremely regular and beautiful, and much superior for most purposes to that produced by the other method. After the grain is formed, every part of the process is conducted in the same manner as above described.

"Having thus given a general idea of the art, we shall mention some particulars necessary to be attended to, in order to ensure success in the operatioh. The spirits of wine must be rectified, and of the best quality: what is sold in the shops contains camphor, which would entirely spoil the grain.

"Resin, Burgundy pitch, and gum-mastic, when dissolved in spirits of wine, produce grains of a different appearance and figure, and are sometimes used separately and sometimes mixed in different proportions, according to the

taste of the artist, some using one substance and some another.

"In order to produce a coarse or fine grain, it is necessary to use a greater or smaller quantity of resin; and to ascertain the proper proportions, several spare pieces of copper must be provided, on which the liquid may be poured, and the grain examined before it is applied to the plate to be engraved.

"After the solution is made, it must stand still and undisturbed for a day or two, till all the impurities of the resin have settled to the bottom, and the fluid is perfectly pellucid. No other method of freeing it from those impurities has been found to answer. Straining it through linen or muslin fills it with hairs, which are ruinous to the grain.

"The room in which the liquid is poured on the plate must be perfectly still, and free from dust, which, whenever it falls on the plate while wet, causes the grain to form a white spot, which it is impossible to remove without laying the grain afresh.

"The plate must be previously cleaned, with the greatest possible care, with a rag and whiting, as the smallest stain or particle of grease produces a streak or blemish in the grain.

"All these attentions are absolutely necessary to produce a tolerable regular grain; and, after every thing that can be done by the most experienced artists, still there is much uncertainty in the process. They are sometimes obliged to lay on the grain several times before they procure one sufficiently regular. The same proportions of materials do not always produce the same effect, as it depends in some degree upon their qualities, and it is even materially affected by the weather. These difficulties are not to be surmounted but by a great deal of experience; and those who are daily in the habit of practising the art are frequently liable to the most unaccountable accidents. Indeed it is much to be lamented, that so elegant and useful a process should be so delicate and uncertain.

"It being necessary to hold the plate in a slanting direction in order to drain off the superfluous fluid, there will naturally be a greater body of the liquid at the bottom than at the top of the plate. On this account, a grain laid in this way is always coarser at that side of the plate that was held lowermost. The most usual way is, to keep the coarsest side for the foreground, being generally the part that has the deepest shadows. In large landscapes, sometimes

times various parts are laid with different grains, according to the nature of the subject.

"The finer the grain is, the more nearly does the impression resemble Indian ink, and the fitter it is for imitating drawings. But very fine grains have several disadvantages: for they are apt to come off before the aquafortis has lain on long enough to produce the desired depth; and as the plate is not corroded so deep, it sooner wears out in printing: whereas coarser grains are firmer, the acid goes deeper, and the plate will throw off a great many more impressions. The reason of all this is evident, when it is considered, that in the fine grains the particles are small and near to each other, and consequently the aquafortis, which acts laterally as well as downwards, soon undermines the particles and causes them to come off. If left too long on the plate, the acid would eat away the grain entirely.

"On these accounts, therefore, the moderately coarse grains are more sought after, and answer better the purpose of the publisher, than the fine grains which were formerly in use.

"Although there are considerable difficulties in laying properly the aquatint grain, yet the corroding the copper, or biting in, so as to produce exactly the tint required, is still more precarious and uncertain. All engravers allow that no positive rules can be laid down by which the success of the process can be secured; nothing but a great deal of experience and attentive observation can enable the artist to do it with any degree of certainty.

"There are some hints, however, which may be of considerable importance to the person who wishes to attain the practice of this art.

"It is evident, that the longer the acid remains on the copper, the deeper it bites, and consequently the darker will be the shade in the impression. It may be of some use, therefore, to have several bits of copper laid with aquatint ground of the same kind that is to be used in the plate, and to let the aquafortis remain for different lengths of time on each; and then to examine the tints produced in one, two, three, four minutes, or longer. Observations of this kind frequently repeated, and with different degrees of strength of the acid, will at length assist the judgment in guessing at the tint which is produced in the plate. A magnifier is also useful to examine the grain, and to observe the depth to which it is bit. It must be observed that no proof of the plate can be obtained till the whole process is finished.

"If any part appears to have been bit too dark, it must

be burnished down with a steel burnisher; and this requires great delicacy and good management, not to make the shade streaky: and the beauty and durability of the grain is always somewhat injured by it, so that it should be avoided as much as possible.

"Those parts which are not dark enough must have a fresh grain laid over them, and be stopped round with varnish and subjected again to the aquafortis. This is called *re-biting*, and requires peculiar care and attention. The plate must be very well cleaned out with turpentine before the grain is laid on, which should be pretty coarse, otherwise it will not lie upon the heights only, as is necessary in order to produce the same grain. If the new grain is different from the former, it will not be so clear, nor so firm, but rotten.

"We have now given a general account of the process of engraving in aquatinta; and we believe that no material circumstance has been omitted that can be communicated without seeing the operation. But after all, it must be confessed that no printed directions whatever can enable a person to practise it. Its success depends upon so many niceties, and attention to circumstances apparently trifling, that the person who attempts it must not be surprised if he does not succeed at first. It is a species of engraving simple and expeditious, if every thing goes on well; but it is very precarious, and the errors which are made are rectified with great difficulty.

"It seems to be adapted chiefly for imitations of sketches, washed drawings, and slight subjects; but does not appear to be at all calculated to produce prints from finished pictures, as it is not susceptible of that accuracy in the balance of tints necessary for this purpose. Nor does it appear to be suited for book plates, as it does not throw off a sufficient number of impressions. It is therefore not to be put into competition with the other modes of engraving. If confined to those subjects for which it is calculated, it must be allowed to be extremely useful, as it is expeditious, and may be attained with much less difficulty than any other mode of engraving. But even this circumstance is a source of mischief, as it occasions the production of a multitude of prints that have no other effect than that of vitiating the public taste."

In the art before described, the artists experience much inconvenience from the quantity of fumes liberated by the action of the acid upon the copper, which, when the plate is large, is very great. To remedy this inconvenience, the

 following

following arrangement, which we think well calculated to answer the purpose, has been suggested to us by Mr. Cornelius Varley, a young artist who distinguishes himself no less by his mechanical abilities than by the exquisite productions of his pencil in water colours:—Get a frame made of common deal or any kind of wood, three or four inches deep, covered with a plate of glass, and open at one side; and let the side opposite to this have a round opening communicating, by means of common iron pipe, with the ash-pit of any little stove or other fire-place, shut up from all other access of air but what must pass through the pipe. It is obvious that any fumes rising from a copper-plate laid under such a frame will be carried backward into the iron pipe by the current of air required to maintain combustion in the stove, and will by this means be carried up the chimney in place of being allowed to fly about in the apartment. The pipe may be very conveniently used by carrying it down through the table to the floor, and so along to the place where the chimney may chance to stand; and when the frame is not wanted, the pipe at one of the joinings (as at A, Plate V.) may be made to answer the purpose of a hinge by which to turn up the frame against the wall, as marked by the dotted lines, where it may be secured, while out of use, by a button or any other contrivance.

XXIX. *On pure Nickel, discovered to be a noble Metal; on its Preparation and Properties. By* J. B. Richter*.

When sulphate of ammonia and nickel are repeatedly crystallized, the whole of the cobalt, excepting a very minute quantity, is separated; but there still remains some copper mixed with the salt. I some time ago announced, that this metal may be separated from the nickel by subliming the latter with sal ammoniac, but I had not then ever obtained pure nickel. With the compound salt of nickel and ammonia there still remains a little arsenic. Iron also may be in it, if we have been a little too sparing in the addition of nitric acid to the sulphuric solution of cobalt containing nickel.

I attempted to separate these extraneous metals in the humid way, but without complete success. By means of carbonate of potash, I decomposed the triple ammoniacal

* From *Van Mons's Journal de Chimie*, vol. vi.

salt

salt of nickel free from iron, and as much so as possible from cobalt; but the precipitate was still visibly of a greenish blue colour. Having edulcorated it and heated it to redness, it parted with its carbonic acid, and changed its colour to a blackish gray, which, however, inclined evidently to a green. The water employed in washing it, which had a greenish appearance, was evaporated to dryness, and the residuum, after being heated red hot, was washed again. A green powder remained, which did not lose its colour in the fire, and consisted chiefly of arseniate of nickel.

Each of the two residuums were separately mixed with a fifth part of charcoal, and, in a cupel with a little porcelain glaze, exposed to the heat of a potter's furnace for eighteen hours. Each result endured a few blows of a hammer without cracking; but that of the latter residuum was much more white and fragile than that of the former, the colour of which approached that of steel, and was slightly reddish. They were both attacked with avidity by nitric acid, and they were attracted by the magnet, but the former only weakly.

As it appeared to me probable from some of its effects on porcelain that pure nickel was a noble metal, I dissolved again, in nitric acid, the whole quantity reduced, which amounted to several ounces, and evaporated the solution to dryness. I then poured water on the saline mass, and a beautiful green solution was formed; but a greenish white residuum remained, in which I easily detected the presence of iron, nickel, and arsenic acid.

This solution, which contained a considerable portion of copper, besides arsenic, was precipitated by carbonate of potash, and the residuum, the colour of which was still very lively, though not so green as that of carbonate of copper, was well washed and exposed to a white heat. This changed its apple-green colour to a deep green inclining to gray and brown. With a stronger heat the mass assumed a grayer brown, and at the same time appeared to coagulate. There were likewise portions of reduced metal in it, that could not be mistaken.—I could not, however, accomplish its fusion in a wind-furnace surmounted with a cupellating furnace dome, and having a long chimney. In consequence, I divided it into several portions, which I exposed in crucibles to the strongest heat of a potter's furnace, in which capsules of the most refractory clay are frequently softened.

In such crucibles as were placed where the porcelain requires

quires the longest time to bake it, the matter underwent no change but that of being coagulated. In the other crucibles the matter had fused, but not so as to be completely liquid: the crucibles themselves had also partly experienced the same effect: here and there in the melted mass metallic globules were found, the largest of which were the size of a small nut, and the least that of a cherry-stone. Their brilliancy was a mean between that of silver and that of English tin. The scoriæ were greenish brown, mixed with an amethyst colour, and in some places a deep blue entirely like fused oxide of cobalt. The brown colour arose from the oxide of copper, which was completely vitrified, and the blue from that of cobalt. The green, on the contrary, proceeded from arseniate of nickel, which, as experience has taught me, strongly resists fusion unless some combustible substance be added to it.

To my great satisfaction on, trying the metallic globules with a hammer, I found that they possessed a considerable degree of malleability.

As I found it impossible to separate with a hammer the scoriæ from the little globules to which they adhered, I collected them together by trituration and decantation, and exposed them to fusion afresh. It was again complete only in the places of the furnace most heated.

These experiments having convinced me that nickel is reducible in the fire, without the addition of any combustible matter, I attempted to reduce some oxide of this metal, obtained by the decomposition of the triple ammoniacal salt of nickel, which during an uninterrupted labour of eighteen months I had procured in a very large quantity. On this occasion the same phænomena occurred as in the preceding reductions.

The melting I repeated till the metal had undergone a complete fusion, and was found collected together in a button at the bottom of the crucible. In one crucible which had been exposed to the strongest heat, I obtained a button that weighed an ounce and a half. I was less successful in my fusion when I mixed the oxide of nickel with porcelain glaze, or when I simply covered it with this glaze; so that I was convinced the best process was to reduce the oxide of nickel directly. After much time and patience, I succeeded in obtaining several ounces of nickel, which I must consider as absolutely pure: and I shall now proceed to describe the principal characters that I have perceived in it in this state.— To begin with the external characters.

The colour of pure nickel is a mean between silver and tin.

It is no way altered either by atmospheric air, or the water it contains. In other words, it is not susceptible of being oxided by the air.

It is so perfectly malleable, that it may not only be forged into bars when red hot, but hammered on the anvil while cold into very thin plates. This character removes nickel from the class of semi-metals to that of perfect metals.

Its density or specific gravity is pretty considerable: from repeated experiments with my hydrometer, cast nickel weighs 8·279, and forged nickel 8·666.

Its tenacity is likewise considerable, if we may judge from its great ductility. A piece of cast nickel, weighing five drachms allowed itself to be flattened, but not without cracking, into a plate of 13 square inches Rhynland measure, which gives less than $\frac{1}{100}$ of an inch for its thickness. It might probably be drawn into a wire of the same tenuity.

The resistance of nickel to fusion is considerable, and equals, if it does not surpass, that of manganese. The reader, from my attempts to fuse it, may have observed how difficult it is to obtain on this head any thing decisive.

When exposed to a sufficiently high temperature, the pure oxide of nickel is reducible without the addition of any combustible matter. Its great resistance to fusion is the only cause why this reduction presents so many difficulties. Very little oxidation, too, is perceptible on keeping this metal in a state of incandescence: it is merely tarnished a little more than platina, gold, or silver. Nickel, therefore, belongs not to the class of perfect metals merely, but to that of noble metals.

The magnet exercises on nickel an action not only very considerable, but little inferior to that on iron. It becomes magnetical, or acquires polarity, by the touch, and even in part by striking it with a hammer, or filing it, with the precautions suitable for producing this effect. I discovered the latter property by presenting to the magnet a slip of forged nickel; when, notwithstanding it was polished by the file, it adhered more feebly to the magnet than other slips less polished; but on my presenting its other extremity to the magnet, it adhered to it with great force. It likewise attracted by either side not only iron needles, but plates of nickel half an inch square, which it caused to move about on a smooth table.

Nickel does not lose its property of becoming magnetic, but has it weakened by being alloyed with copper. Arsenic, however, completely destroys it. I had frequent opportu-

nities

nities of making this observation in the course of my experiments. Some nickel, from which I had separated the iron* and the arsenic in the humid way, and which I had afterwards reduced with the addition of a combustible substance, was malleable, and attracted the magnet, though not so forcibly as pure nickel. The same metal, purified with less care, was less malleable, and proportionally less attractable by the magnet. Repeated exposure of the metal to the most powerful heat of a porcelain furnace did not in the least restore to it this property.—Some experiments, which I shall hereafter relate, have convinced me, that copper cannot be entirely separated from nickel in the humid way, and that the only means of separating them is to reduce the cupreous oxide of nickel by fire.

The sulphuric and muriatic acids exercise but little action on nickel. Even its oxide by air does not dissolve in the latter without the assistance of a strong ebullition. The most appropriate solvents of nickel are the nitric and nitro-muriatic acids. I have already mentioned, that impure nickel, particularly the cupreous, is attacked by the nitric acid with heat and vivacity. The action of the same acid on pure nickel is a little different, and particularly on the hammered metal. I have poured nitric acid on nickel both in buttons and laminated, expecting a very active solution; but it has proceeded slowly, and I have even been obliged to have recourse to the heat of a spirit lamp to accelerate it. The dissolution, however, having appeared to cease, I decanted the liquid and poured on the residuum a fresh quantity of acid of the same strength as the preceding, when on a sudden such a brisk action came on, accompanied with the evolution of heat†, that I could not remove the capsule to the fire-place quickly enough.

I shall now proceed to consider some of the characters of pure nickel in the state of oxidation.

The nitric solution of pure nickel has a beautiful grass-green colour. Carbonate of potash separates from it a pale apple-green precipitate. This precipitate well washed and dried is very light. A thousand parts of metallic nickel reduced to this precipitate weigh 2·927 parts.

* The separation of the iron succeeds best by a rapid evaporation of the nitric solution of the ferruginous nickel, by which the iron is precipitated in the form of an insoluble oxide. At the same time a little arsenic is separated in union with the iron. It is preferable, however, to separate the arsenic first, which is effected by the help of a nitric solution of lead. The lead is afterwards to be precipitated by a solution of sulphate of potash.

† From this it is difficult to believe that nickel, under favourable circumstances, would not become oxided by the combined influence of air and fire.—*Van. Mons.*

If

If this precipitate be exposed to a white heat it becomes of a blackish gray, scarcely inclining to green, and weighing only 1·285. On continuing the fire, the mass approaches the metallic state more and more, and becomes magnetic. This is effected much more speedily if the oxide be moistened with a little oil.

If caustic ammonia be added in excess to a nitric solution of nickel, a precipitate is formed, resembling in colour ammoniure of copper, but not so deep. This colour sometimes changes in a couple of hours to an amethyst-red, and to a violet, which colours are converted into an apple-green on the addition of an acid, and again to a blue and violet on the addition of ammonia. If, however, we add to the solution of nickel a solution of copper, so as to produce no perceptible change, the colour of the precipitate formed by ammonia ceases to assume a red tinge, and the red colour of the ammoniure of nickel disappears on the addition of a little ammoniure of copper; whence it follows, that every precipitate of nickel by ammonia which retains its blue colour, has copper combined with it.

XXX. *Account of a new Vegetable Substance discovered by* M. Rose*.

After standing some hours, a decoction of the root of elecampane (*inula helenium*) deposits a white powder, appearing very much like starch, but differing from it both in its principles and in its manner of action with other substances.

1. This substance is insoluble, generally, in cold water:-- When triturated with it a white milky liquor is formed, which soon deposits a heavy white powder, and leaves the supernatant water clear and colourless.

2. In boiling water it dissolves very well. On boiling one part of the white powder with four parts of water, a complete solution is obtained which passes through filtering paper while hot, but, on cooling, acquires a mucilaginous consistence and a dull colour. In the course of some hours this solution deposits the greater part of the dissolved substance in the form of a compact white powder.

One part of gum-arabic, dissolved in four parts of water, is much thicker, of a more tenacious consistence, and froths lightly, which the solution of the powder from the elecampane root does not.

3. The solution of the white powder mixed with an equal quantity of alcohol is at first clear, but in a little time the powder separates in the form of a tumid white sediment, leaving the fluid above it transparent. A solution of gum-

* From *Gehlen's Journal*, vol. iii.

arabic on the addition of alcohol becomes immediately milky, and long retains this appearance, no kind of powder separating even in several days.

4. The white powder, when thrown on burning coals, melts like sugar and evaporates, diffusing a white, thick, pungent smoke, with a smell of burnt sugar. After this combustion a light residuum only remains, which runs into the coal. Starch emits a similar smoke, but does not melt, and leaves a coally residuum much greater in quantity. Gum-arabic under the same circumstances gives out scarcely any smoke.

When heated over charcoal in an iron spoon, the powder first melts and emits the smoke above described; but as soon as the spoon becomes red hot, it burns with a vivid light flame, and leaves a very trifling coally residuum. Starch under the same circumstances does not melt, is much longer before it burns, and leaves a considerable residuum of coally matter. Gum-arabic only sparkles, does not take fire, and leaves a great deal of coal, which is readily convertible into grayish ashes.

5. From this powder, by dry distillation, we obtain a brown empyreumatic acid, having the smell of pyroxalic acid, but none of empyreumatic oil.

6. Nitric acid transforms the powder into malic and oxalic acids, without producing a single atom of saccholactic acid, which gum-arabic furnishes very abundantly when treated in the same manner; nor does it yield any of the fatty matter generated by the action of nitric acid on starch.

It follows from all these phænomena, that this farinaceous powder extracted from elecampane root is neither starch nor gum, but a peculiar vegetable substance holding a middle rank between the two. It is probable that it exists in many other vegetables, and that several products hitherto considered as starch are of the same nature as this farina.

XXXI. *Facts relative to the Torpid State of the North-American Alligator. By* BENJAMIN SMITH BARTON, M.D*.

It has not, I think, been remarked by the generality of the writers on natural history, that the North American Alligator passes during the prevalence of cold weather into the torpid state. This, however, is unquestionably the case in some parts of the continent.

* From *The Philadelphia Medical and Physical Journal*, edited by Dr. Barton.

Mr.

Mr. Bossu, a French writer, after telling us that these animals are numerous in the Red River, one of the western branches of the Mississippi, says "they are torpid during the cold weather, and lie in the mud with their mouths open, into which the fish enter as into a funnel, and neither advance nor go back. The Indians then get upon their backs, and kill them, by striking their heads with hatchets, and this is a kind of diversion for them*."

Dr. Foster, the translator of the work, observes in the preceding passage, "that the circumstance of the alligator's being torpid during winter is quite new, and very remarkable for natural history." It seems (he adds) almost all the class of animals called *amphibia*, by Dr. Linnæus, when found in cold climates grow torpid during winter.

In addition to the authority of Mr. Bossu, I may here mention the following fact, which was communicated to me about the year 1785, by a Mr. Graham, at that time a very intelligent student of medicine in the University of Pennsylvania.

"The alligator, having previously swallowed a number of pine-knots, retires to his hole, where he remains in a torpid state during the severity of the winter. If killed at this season, these knots are found highly polished by their trituration one against the other in the animal's stomach, as I have more than once heard from men of undoubted veracity, who had been concerned in the fact. Indeed this is so notorious in those parts in which these creatures abound, that the digestion of the alligator's stomach is proverbial amongst the multitude, who deride its insipidity in the choice of such food, though, I presume, this it does instinctively, for some purpose unaccounted for by naturalists; and which, perhaps, is beyond the limits of human ken."

The fact related by Mr. Graham, relates to the alligator of the Carolinas, in which parts of the United States this animal is very common. By another gentleman I have been informed, that the pine-knots which the alligators swallow are generally such as are very abundant in turpentine. I have also been assured, by my friend Mr. William Bartram, that he has seen a brick-bat which was taken out of the stomach of an alligator, and that it was worn quite round.

Mr. Lawson says, that the alligator is not seen to the north of North-Carolina. They are very common at Cape-Fear in latitude 34°. One twelve feet in length has been

* Travels through that Part of North America formerly called Louisiana. English translation, vol. i. p. 367. London 1771.

seen

seen at this place. On the Atlantic side of the United States I am not able to trace them further than the "Alligator Dismal Swamp", which is between Edenton and Newbern in North-Carolina. The mouth of the Red River in latitude 31°.

Within the tract of country just mentioned, the alligator, obeying the impulse of the climate, passes into the torpid state. In North-Carolina this takes place about the middle of November, sooner or later according to the state of the season. Whether the animal becomes torpid in more southern parts of the continent I have not been able to learn. On the river St. John, in East-Florida, they have been seen awake even in the middle of winter, but it was remarked that they seemed dull and stupid. It has also been observed, that they are accustomed to frequent the warm springs which are so abundant in this part of the continent; and that they are fond of lying in these springs. Perhaps the heat of these springs may be sufficient to prevent them from becoming torpid. But it must be observed, that a deficiency of heat is not the only cause of the torpid condition of animals.

It may not perhaps be an easy task to assign a satisfactory cause for the singular instinctive appetite which leads the alligator, before going into the torpid state, to swallow pine-knots and other somewhat similar substances. But I apprehend that these substances, when taken in by the animal, act in some measure by keeping up a certain degree of action in its stomach, and consequently in every part of the system, and thereby prevents the death of the animal, which might otherwise be destroyed by the long continued application of cold. Some facts mentioned by Dr. Pallas, though they respect a very different family of animals, render this conjecture not a little plausible*.

This subject is worthy of more attention. In particular, it will be well to inquire, whether the alligator does swallow pine-knots, stones, &c., in those parts of America in which it does not pass into the torpid state.

XXXII. *Process for obtaining pure Cobalt. By M.* Tromsdorff †.

Mix intimately four parts of zaffer, well pulverized, with one part of nitrate of potash and a half part of charcoal in

* Historia Glirium, &c.

† Extracted from the *Journal de Chimie* of M. Gehlen by M. Darcet, and inserted in the *Annales de Chimie*, No. 162.

 powder:

powder: throw this mixture, by little and little, into a red-hot crucible, and repeat the operation three times, adding each time to the residue new quantities of potash and of charcoal.

The mass resulting from these detonations ought afterwards to be mixed with one part of black flux, and exposed during an hour, in a crucible, to a red heat. Leave the whole to cool, separate the metallic cobalt and pulverize it, and bring the mixture to detonate with the same precautions indicated above.

The iron contained in the cobalt oxidates itself very strongly, and the acidified arsenic combines with the potash. Wash several times the pulverized mass, and by filtration separate the arseniate of potash, which has been formed, from the insoluble residue which contains the cobalt.

Treat this residue with nitric acid, which dissolves the cobalt without attacking the iron, which is carried to its maximum of oxidation.

Evaporate the solution to dryness; redissolve the residue in the nitric acid, and filter the liquor to separate the last portions of oxide of iron which might have escaped at the first operation.

It only remains, then, to decompose the nitrate of cobalt by potash, to wash the precipitate, and to operate the reduction of it by means of heat.

XXXIII. *On a Method of analysing Stones containing fixed Alkali, by Means of the Boracic Acid.* By HUMPHRY DAVY, *Esq. F.R.S., Professor of Chemistry in the Royal Institution**.

I HAVE found the boracic acid a very useful substance for bringing the constituent parts of stones containing a fixed alkali into solution.

Its attraction for the different simple earths is considerable at the heat of ignition, but the compounds that it forms with them are easily decomposed by the mineral acids dissolved in water, and it is on this circumstance that the method of analysis is founded.

The processes are very simple.

* From the *Transactions of the Royal Society* for 1805.

100 grains

100 grains of the stone to be examined in very fine powder, must be fused for about half an hour, at a strong red heat, in a crucible of platina or silver, with 200 grains of boracic acid.

An ounce and half of nitric acid, diluted with seven or eight times its quantity of water, must be digested upon the fused mass till the whole is decomposed.

The fluid must be evaporated till its quantity is reduced to an ounce and half or two ounces.

If the stone contain silex, this earth will be separated in the process of solution and evaporation; and it must be collected upon a filter, and washed with distilled water till the boracic acid and all the saline matter is separated from it.

The fluid, mixed with the water that has passed through the filter, must be evaporated, till it is reduced to a convenient quantity, such as that of half a pint; when it must be saturated with carbonate of ammonia, and boiled with an excess of this salt, till all the materials that it contains, capable of being precipitated, have fallen to the bottom of the vessel.

The solution must then be separated by the filter, and the earths and metallic oxides retained.

It must be mixed with nitric acid till it tastes strongly sour, and evaporated till the boracic acid appears free.

The fluid must be passed through the filter, and subjected to evaporation till it becomes dry; when, by exposure to a heat equal to 450° Fahrenheit, the nitrate of ammonia will be decomposed, and the nitrate of potash or soda will remain in the vessel.

It will be unnecessary for me to describe minutely the method of obtaining the remaining earths and metallic oxides free from each other, as I have used the common processes. I have separated the alumine by solution of potash, the lime by sulphuric acid, the oxide of iron by succinate of ammonia, the manganese by hydrosulphuret of potash, and the magnesia by pure soda.

XXXIV. *Observations on the singular Figure of the Planet Saturn.* *By* WILLIAM HERSCHELL, *LL.D. F.R.S.**

THERE is not perhaps another object in the heavens that presents us with such a variety of extraordinary phænomena

* From the *Transactions of the Royal Society* for 1805.

as the planet Saturn: a magnificent globe, encompassed by a stupendous double ring; attended by seven satellites; ornamented with equatorial belts; compressed at the poles; turning upon its axis; mutually eclipsing its ring and satellites, and eclipsed by them; the most distant of the rings also turning upon its axis, and the same taking place with the furthest of the satellites; all the parts of the system of Saturn occasionally reflecting light to each other; the rings and moons illuminating the nights of the Saturnian; the globe and satellites enlightening the dark parts of the rings; and the planet and rings throwing back the sun's beams upon the moons, when they are deprived of them at the time of their conjunctions.

It must be confessed that a detail of circumstances like these appears to leave hardly any room for addition, and yet the following observations will prove that there is a singularity left, which distinguishes the figure of Saturn from that of all the other planets.

It has already been mentioned on a former occasion, that so far back as the year 1776 I perceived that the body of Saturn was not exactly round; and when I found in the year 1781 that it was flattened at the poles at least as much as Jupiter, I was insensibly diverted from a more critical attention to the rest of the figure. Prepossessed with its being spheroidical, I measured the equatorial and polar diameters in the year 1789, and supposed there could be no other particularity to remark in the figure of the planet. When I perceived a certain irregularity in other parts of the body, it was generally ascribed to the interference of the ring, which prevents a complete view of its whole contour; and in this error I might still have remained, had not a late examination of the powers of my 10-feet telescope convinced me that I ought to rely with the greatest confidence upon the truth of its representations of the most minute objects I inspected.

The following observations, in which the singular figure of Saturn is fully investigated, contain many remarks on the rest of the appearances that may be seen when this beautiful planet is examined with attention; and though they are not immediately necessary to my present subject, I thought it right to retain them, as they show the degree of distinctness and precision of the action of the telescope, and the clearness of the atmosphere at the time of observation.

April 12, 1805. With a new 7-feet mirror of extraordinary

dinary distinctness, I examined the planet Saturn. The ring reflects more light than the body, and with a power of 570 the colour of the body becomes yellowish, while that of the ring remains more white. This gives us an opportunity to distinguish the ring from the body, in that part where it crosses the disk, by means of the difference in the colour of the reflected light. I saw the quintuple belt, and the flattening of the body at the polar regions; I could also perceive the vacant space between the two rings.

The flattening of the polar regions is not in that gradual manner as with Jupiter, it seems not to begin till at a high latitude, and there to be more sudden than it is towards the poles of Jupiter. I have often made the same observation before, but do not remember to have recorded it any where.

April 18; 10-feet reflector, power 300. The air is very favourable, and I see the planet extremely well defined. The shadow of the ring is very black in its extent over the disk south of the ring, where I see it all the way with great distinctness.

The usual belts are on the body of Saturn; they cover a much larger zone than the belts on Jupiter generally take up, as may be seen in the figure I have given in Plate IX.; and also in a former representation of the same belts in 1794*.

The figure of the body of Saturn, as I see it at present, is certainly different from the spheroidical figure of Jupiter. The curvature is greatest in a high latitude.

I took a measure of the situation of the four points of the greatest curvature, with my angular micrometer, and power 527. When the cross of the micrometer passed through all the four points, the angle which gives the double latitude of two of the points, one being north, the other south of the ring, or equator, was 93° 16′. The latitude therefore of the four points is 46° 38′; it is there the greatest curvature takes place. As neither of the cross wires can be in the parallel, it makes the measure so difficult to take, that very great accuracy cannot be expected.

The most northern belt comes up to the place where the ring of Saturn passes behind the body, but the belt is bent in a contrary direction, being concave to the north, on account of its crossing the body on the side turned towards us, and the north pole being in view.

There is a very dark, but narrow shadow of the body

* See Phil. Trans. for 1794, Table VI. page 32.

upon the following part of the ring, which as it were cuts off the ring from the body.

The shadow of the ring on the body, which I see south of the ring, grows a little broader on both sides near the margin of the disk.

The division between the two rings is dark, like the vacant space between the ansæ, but not black like the shadow I have described.

There are four satellites on the preceding side near the ring; the largest and another are north-preceding; the other two are nearly preceding.

April 19. I viewed the planet Saturn with a new 7-feet telescope, both mirrors of which are very perfect. I saw all the phænomena as described last night, except the satellites, which had changed their situation; four of them being on the following side. This telescope however is not equal to the 10-feet one.

The remarkable figure of Saturn admits of no doubt: when our particular attention is once drawn to an object, we see things at first sight that would otherwise have escaped our notice.

10-feet reflector, power 400. The night is beautifully clear, and the planet near the meridian. The figure of Saturn is somewhat like a square or rather parallelogram, with the four corners rounded off deeply, but not so much as to bring it to a spheroid. I see it in perfection.

The four satellites that were last night on the preceding, are now on the following side, and are very bright.

I took a measure of the position of the four points of the greatest curvature, and found it 91° 29′. This gives their latitude 45° 44′,5. I believe this measure to be pretty accurate. I set first the fixed thread to one of the lines, by keeping the north-preceding and south-following two points in the thread; then adjusted the other thread in the same manner to the south-preceding and north-following points.

May 5, 1805. I directed my 20-feet telescope to Saturn, and, with a power of about 300, saw the planet perfectly well defined, the evening being remarkably clear. The shadow of the ring on the body is quite black. All the other phænomena are very distinct.

The figure of the planet is certainly not spheroidical, like that of Mars and Jupiter. The curvature is less on the equator and on the poles than at the latitude of about 45 degrees. The equatorial diameter is however considerably greater than the polar.

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In order to have the testimony of all my instruments, on the subject of the structure of the planet Saturn, I had prepared the 40-feet reflector for observing it in the meridian. I used a magnifying power of 360, and saw its form exactly as I had seen it in the 10- and 20-feet instruments. The planet is flattened at the poles, but the spheroid that would arise from this flattening is modified by some other cause, which I suppose to be the attraction of the ring. It resembles a parallelogram, one side whereof is the equatorial, the other the polar diameter, with the four corners rounded off so as to leave both the equatorial and polar regions flatter than they would be in a regular spheroidical figure.

The planet Jupiter being by this time got up to a considerable altitude, I viewed it alternately with Saturn in the 10-feet reflector, with a power of 500. The outlines of the figure of Saturn are as described in the observation of the 40-feet telescope; but those of Jupiter are such as to give a greater curvature both to the polar and equatorial regions than takes place at the poles or equator of Saturn which are comparatively much flatter.

May 12. I viewed Saturn and Jupiter alternately with my large 10-feet telescope of 24 inches aperture: and saw plainly that the former planet differs much in figure from the latter.

The temperature of the air is so changeable that no large mirror can act well.

May 13. 10-feet reflector, power 300. The shadows of the ring upon the body, and of the body upon the ring, are very black, and not of the dusky colour of the heavens about the planet, or of the space between the ring and planet, and between the two rings. The north-following part of the ring, close to the planet, is as it were cut off by the shadow of the body: and the shadow of the ring lies south of it, but close to the projection of the ring.

The planet is of the form described in the observation of the 40-feet telescope; I see it so distinctly that there can be no doubt of it. By the appearance I should think the points of the greatest curvature not to be so far north as 45 degrees.

The evening being very calm and clear, I took a measure of their situation, which gives the latitude of the greatest curvature 45° 21′. A second measure gives 45° 41′.

Jupiter being now at a considerable altitude, I have viewed it alternately with Saturn. The figure of the two planets is decidedly different. The flattening at the poles and on the

equator of Saturn is much greater than it is on Jupiter, but the curvature at the latitude of from 40 to 48° on Jupiter is less than on Saturn.

I repeated these alternate observations many times, and the oftener I compared the two planets together, the more striking was their different structure.

May 26. 10-feet reflector. With a parallel thread micrometer and a magnifying power of 400, I took two measures of the diameter of the points of greatest curvature. A mean of them gave 64,3 divisions = 11″,98. After this, I took also two measures of the equatorial diameter, and a mean of them gave 60,5 divisions = 11″,27; but the equatorial measures are probably too small.

To judge by a view of the planet, I should suppose the latitude of the greatest curvature to be less than 45 degrees. The eye will also distinguish the difference in the three diameters of Saturn. That which passes through the points of the greatest curvature is the largest; the equatorial the next, and the polar diameter is the smallest.

May 27. The evening being very favourable, I took again two measures of the diameter between the points of greatest curvature, a mean of which was 63,8 divisions = 11″,88. Two measures of the equatorial diameter gave 61,3 divisions = 11″,44.

June 1. It occurred to me that a more accurate measure might be had of the latitude in which the greatest curvature takes place, by setting the fixed thread of the micrometer to the direction of the ring of Saturn, which may be done with great accuracy. The two following measures were taken in this manner, and are more satisfactory than I had taken before. The first gave the latitude of the south-preceding point of greatest curvature 43° 26′; and the second 43° 13′. A mean of the two will be 43° 20′.

June 2. I viewed Jupiter and Saturn alternately with a magnifying power of only 300, that the convexity of the eye-glass might occasion no deception, and found the form of the two planets to differ in the manner that has been described.

With 200 I saw the difference very plainly; and even with 160 it was sufficiently visible to admit of no doubt. These low powers show the figure of the planets perfectly well, for as the field of view is enlarged, and the motion of the objects in passing it lessened, we are more at liberty to fix our attention upon them.

I compared the telescopic appearance of Saturn with a figure drawn by the measures I have taken, combined with the

the proportion between the equatorial and polar diameters determined in the year 1789*; and found that, in order to be a perfect resemblance, my figure required some small reduction of the longest diameter, so as to bring it nearly to agree with the measures taken the 27th of May. When I had made the necessary alteration, my artificial Saturn was again compared with the telescopic representation of the planet, and I was then satisfied that it had all the correctness of which a judgment of the eye is capable. An exact copy of it is given in Plate VII. The dimensions of it in proportional parts are,

The diameter of the greatest curvature -	36
The equatorial diameter - -	35
The polar diameter - - -	32
Latitude of the longest diameter -	43° 20′

The foregoing observations of the figure of the body of Saturn, will lead to some intricate researches, by which the quantity of matter in the ring, and its solidity, may be in some measure ascertained. They also afford a new instance of the effect of gravitation on the figure of planets; for in the case of Saturn, we shall have to consider the opposite influence of two centripetal and two centrifugal forces: the rotation of both the ring and planet having been ascertained in some of my former papers.

XXXV. *Experiments and Observations upon the Contraction of Water by Heat at low Temperatures.* *By* Thomas Charles Hope, *M.D. F.R.S. Edin., Professor of Chemistry in the University of Edinburgh*†.

To the general law, that bodies are expanded by heat, and contracted by cold, water at the point of congelation, and for some degrees of temperature above it, seems to afford a very singular and curious exception.

The circumstances of this remarkable anomaly have been for some time believed to be the following:

When heat is applied to water ice-cold, or at a temperature not far distant, it causes a diminution in the bulk of the fluid. The water contracts, and continues to contract, with the augmentation of temperature, till it reaches the

* See Phil. Trans for 1790, p. 17.

† From the *Transactions of the Royal Society of Edinburgh*, part ii. of vol. v.

40th

40th or 41st degree. Between this point and the 42d or 43d, it suffers scarcely any perceptible change; but when heated beyond the last-mentioned degree it begins to expand, and increases in volume with every subsequent rise of temperature.

During the abstraction of caloric, the peculiarity in the constitution of water equally appears. Warm water, as it cools, shrinks, as other bodies do, till it arrives at the temperature of 43° or 42°. It then suffers a loss of two degrees without any alteration of density. But when further cooled, it begins to dilate, and continues to dilate, as the temperature falls, till congelation actually commences, whether this occurs as soon as the water reaches the 32d degree, or after it has descended any number of degrees below it.

Supposing this peculiarity of water to be established, it must appear, indeed, a very odd circumstance, that heat should produce contraction in this fluid, while it causes expansion in other bodies; and no less strange, that within one range of temperature it should contract, and in another expand, the very same substance. Before a deviation from so general a law should be received as matter of fact, the proofs of its existence ought to be clear and indisputable.

The experiments hitherto published, from which this singularity has been deduced, have all of them been performed upon water contained in instruments shaped like a thermometer glass, and consisting of a ball with a slender stem; and the expansive or contractive effects of heat and cold have been inferred from the ascent or descent of the fluid in the stem.

To such experiments it has been objected, that the dimensions and capacity of the instrument undergo so much change, from variation of temperature, that it is difficult, if not impossible, to determine how much of the apparent anomaly ought to be imputed to such changes, and that it is not improbable that the whole of it may be ascribed to them.

The object of this paper, which I have now the honour to read to the society, is to prove, by a set of experiments, conducted in a manner altogether different, that the common opinion is founded in truth, and that water presents itself as that strange and unaccountable anomaly which I have already described.

It is worth while, before detailing my experiments, to give a short account of those observations which led to the discovery of the fact, and which in succession have extended our knowledge of it, as well as of those observations which have

have at different periods been offered to discredit, and to bring it into doubt.

The first observation relative to this subject was made by Dr. Croune, towards the close of the 17th century, while engaged in investigating the phænomena of the great and forcible, though familiar, expansion which happens to water at the instant of freezing; a matter which occupied, in a considerable degree, the attention of his fellow-members of the Royal Society of London in the earlier years of that institution.

I shall relate, in his own words, his first observation:— "I filled a strong bolt-head about half way up the stem with water, a day or two before the great frost went off, marking the place where the water stood; and placing it in the snow on my leads, while I went to put some salt to the snow, I found it above the mark so soon, that I thought the mark had slipt down, which I presently raised to the water, and as soon as ever I mixed the salt with the snow, the water rose very fast, about one-half inch above it. I took up then the glass, and found the water all fluid still: it was again set down in the salt and snow; but when I came, about an hour after, to view it, the ball was broken, and the water turned to hard ice, both in the ball and stem *."

From this experiment Dr. Croune drew the conclusion, that water, when subjected to cold, actually began to expand before it began to freeze. On announcing it, however, to the Royal Society, on the 6th of February 1683, Dr. Hooke immediately expressed strong doubts, and ascribed the ascent of the water in the neck of the vessel to the shrinking of the glass occasioned by the cold.

To obviate this objection, and to preclude, as far as was possible, the influence of the change of capacity in the apparatus from an alteration of its temperature, a bolt-head was immersed in a mixture of salt and snow, and into it, when cooled, was poured, to a certain height, water previously brought to near the freezing point. The water began instantly to rise as before, and when it had ascended about one-fourth of an inch in the stem, the vessel was taken out, the whole water remaining fluid.

These experiments, supported by others of a similar nature, communicated by Dr. Slare to the society on the 20th of the same month, appear to have satisfied its members, in general, of this fact, that water, when on the point of

* Birch's History of the Royal Society, vol. iv. p. 263.

congealing,

congealing, and while still fluid, is actually somewhat dilated previous to the remarkable expansion which accompanies its conversion into ice.

Dr. Hooke, however, continued unshaken, and retained the doubts he had expressed.

Remarkable as the fact, as now stated, must have appeared, it seems not to have excited particular attention, nor to have solicited more minute examination; and, indeed, though philosophers did not lose sight of it, yet for near a century no one investigated it more carefully. Mairan, in his Treatise on Ice in 1749, and Du Crest in his Dissertation on Thermometers in 1757, appear to be well aware of this property of water; but it is to M. de Luc that we owe the knowledge of the leading and more interesting circumstances. (Vide *Recherches*, &c. 1772.)

Having devoted his attention to the examination and improvement of the thermometer, he was naturally led to the investigation while engaged in ascertaining the phænomena of the expansion and contraction of different fluids by heat and cold.

He employed in his experiments thermometer glasses; and the included water, at or near the term of liquefaction, descended in the stem, and appeared to him to suffer a diminution of bulk by every increase of temperature till it arrived at the 41°. From this point its volume increased with its temperature, and it ascended in the tube. This fluid, when heated and allowed to cool, seemed to him to contract in the ordinary way, till its temperature sunk to the 41°, but to expand and increase in volume as the temperature fell to the freezing point.

The density of water, he thence inferred, is at its maximum at 41°, and decreases with equal certainty whether the temperature is elevated or depressed.

M. de Luc says, indeed, that very nearly the same alteration in volume is occasioned in water of temperature 41°, by a variation of any given number of degrees of temperature, whether they be of increase or of diminution; and consequently, that the density of water at temperature 50°, and at temperature 32°, is the same.

This philosopher did not conceive that the constitution of water, in relation to caloric, undergoes a change at the temperature of 41°, such that short of this degree caloric should occasion contraction, and beyond it expansion. He imagined that heat in all temperatures tends to produce two, but quite opposite, effects on this fluid, the one expansion, the other contraction.

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In low temperatures, the contractive effects surpass the expansive, and contraction is the consequence: in temperatures beyond 41°, the expansive predominate, and the visible expansion is the excess of the expansive operation over the contractive.

In 1788 sir Charles Blagden added the curious observations, that water, which by slow and undisturbed refrigeration permits its temperature to fall many degrees below its freezing point, perseveres in expanding gradually as the temperature declines; and that water, having some muriate of soda, or sea salt, dissolved in it, begins to expand about the same number of degrees above its own term of congelation that the expansion of pure water precedes its freezing, that is, between eight and nine degrees. More lately (Philosophical Transactions 1801), he, or rather Mr. Gilpin by his direction, endeavoured to ascertain, by the balance and weighing-bottle, the amount of this change of density caused by a few degrees of temperature.

Every one must be familiar with the use which count Rumford has made of this peculiarity in the constitution of water, in explaining many curious appearances that presented themselves in his experiments upon the conducting power of fluids, and in accounting for certain remarkable natural occurrences. The count, with his usual ingenuity, has endeavoured to point out the important purposes which this peculiarity serves in the œconomy of nature, and to assign the final cause of so remarkable an exception from a general law.

In recording the observations and opinions that have been published concerning this point, I might now, in order, notice those of Mr. Dalton, of Manchester, related in the fifth volume of the Manchester Memoirs, which tended to confirm and enlarge our knowledge of it. But as Mr. Dalton himself has called in question the accuracy of the conclusions which had been drawn from his experiments, and from those of preceding observers, I shall only remark, that they are of the same nature, and nearly to the same purport, as those of M. de Luc.

It was in consequence of a communication with which Mr. Dalton favoured me, three months ago, that my attention was directed to this subject. He informed me, that after a long train of experiments he was led to believe that he, and his predecessors in the same field of investigation, had fallen into a mistake with regard to the contraction of water by heat, and its expansion by cold, in consequence of overlooking or underrating the effect which the change

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in the capacity of the thermometer-shaped apparatus employed, must occasion on the apparent volume of the fluid. He stated, in general terms, that on subjecting water to different degrees of temperature, in instruments made of different materials, he found the point of greatest density was indicated at a different temperature in each.

In an apparatus, having a ball of earthen ware, it was at the 34th degree; of glass at the 42d; of brass at the 46th; and of lead at the 50th. And as water could not follow a different law, according to the nature of the substance of the instrument, he conceived that the appearance of anomaly in this fluid originated entirely in the containing vessel, which must cause the fluid in the stem to fall or rise according as its expansions are greater or less than those of the included liquor.

A detail of these important experiments has, ere now, been transmitted for publication in the Journals of the Royal Institution of London.

I have already noticed that Dr. Hooke endeavoured to explain in the same manner the original experiment of Dr. Croune. This explanation apparently gathers much force from these experiments of Mr. Dalton.

It is proper, however, to state, that M. de Luc was perfectly aware of the alteration in the dimensions of his glass apparatus, but deemed the change too trifling to have any material influence.

Sir Charles Blagden paid greater attention to the circumstance, and by calculation attempted to appreciate what allowance ought to be made for the change of capacity in the amount of the apparent changes of volume.

When it is considered that the whole amount of the apparent change is but very small, and that the expansibility of the glass is with difficulty ascertained, and is variable by reason of the fluctuating proportions of its heterogeneous constituents, it must be acknowledged that precision in such a calculation cannot possibly be attained, and can scarcely be approached. On this account, all the experiments already noticed are open to the explanation of Dr. Hooke, and in some measure liable to the objection which he had urged. I confess that the experiments of Mr. Dalton, in perfect concurrence with that explanation, created considerable doubts respecting the existence of the peculiarity of water; against the probability of which circumstance, all analogical reasoning, and every argument *à priori*, strongly militate.

Unwilling to remain in uncertainty, and considering it

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as a point of much curiosity and interest, I have endeavoured to investigate the subject by experiments conducted in a totally different manner, equally calculated to exhibit the singular truth, but free from the objections to which the others are liable. In them, it was my object to provide, that neither the changes of the actual volume of the water, nor the alterations in the dimensions of the instrument, should have any influence whatever.

I have already taken occasion to state, that the purpose of this paper is to prove, by experiments on the principle now mentioned, that in the constitution of water there really exists the singularity often noticed.

I shall first state the plan of the experiments, and then detail the particulars of the most remarkable of them.

When any body is dilated, whether by heat or cold, it necessarily becomes less dense, or specifically lighter; and the opposite effects result from contraction. This is the circumstance, as every one knows, which causes various movements among the particles of fluids, when any inequality of temperature prevails in the mass; hence these particles are little acquainted with a state of rest.

If a partial application or subtraction of heat produce an inequality of density in a mass of fluid, the lighter parts rise to the surface, or the denser fall to the bottom.

It readily occurred, that I might avail myself of these movements, and upon statical principles determine the question in dispute.

I had only to examine attentively water, as it was heated or cooled in a jar, and to observe, by means of thermometers, what situation the warmer, and what the cooler parts of this fluid affected.

If I should find that ice-cold water, in acquiring temperature, showed, in its whole progress, the warmer parts near the top, it would indicate that water follows the usual law, and is expanded like other bodies by heat.

Or if I should observe that warm water, in cooling to the freezing point, had the coldest portion uniformly at the bottom, the same conclusion would follow; while a different inference, and the existence of the supposed anomaly, would be deducible should the event prove different. The only circumstance I can figure to myself as tending in any measure to render this mode of examining the point doubtful, is, that water near its congealing point may have so little change of density occasioned by a small variation of temperature, that its particles may be prevented by their inertia, or by the tenacity of the circumfluent mass, from

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assuming that situation which their specific gravity would allot to them.

It will appear, however, very clear, from the circumstances of the experiments which I shall immediately detail, that no obstacle to the success and precision of the experiments proceeded from this source.

It is not necessary for me to relate all the experiments I have made. I shall restrict myself to the detail of six, which present varieties in the modes of procedure, and which afford the most striking results.

Experiment I.

I filled a cylindrical jar of glass 8½ inches deep, and 4½ in diameter, with water of temperature 32°, and placed it on a table, interposing a considerable thickness of matter possessed of little power of conducting heat. I suspended two thermometers in the fluid, nearly in the axis of the jar, one with its ball about half an inch from the bottom, the other at the same distance below the surface. The jar was freely exposed to the air of the room, the temperature of which was from 60° to 62°.

The experiment commenced at noon:

	Top Thermom.	Bottom do.
	32°	32°
In 10 minutes,	33+	34+
— 30 ———	35·5	37
— 50 ———	37	38+
— an hour,	38	38+
— ——— and 10 minutes	42	38·25
— ——— — 30 ———	44	40
— ——— — 50 ———	46+	41+
— 2 hours and 10 minutes	48	42·5
— ——— 30 ———	50	44
— ——— 50 ———	50·5	45
— 4 hours	54	49

Confiding in the indications of the thermometers, from this experiment, we learn, that when heat flows on all sides from the ambient air into a column of ice-cold water, the warmer portions of the fluid actually descend, and take possession of the bottom of the vessel.

This downward course proclaims an increased density, and testifies that the cold water is contracted by heat. As soon, however, as the fluid at the bottom exhibits a temperature of 38°, this course is retarded and soon stopped, and with the rise of temperature beyond 40° is totally changed: for when the mass attains this degree, the experiment

riment equally shows, that the warmer fluid ascends and occupies the summit, by its route announcing its diminished density, and proving that water is now expanded by heat.

Experiment II.

I filled the same jar with water of temperature 53°; and that I might observe the phænomena of cooling, I placed it in the axis of a much larger cylindrical vessel, nearly full of water, of temperature 41°, and, by an earthen-ware support, raised it about three inches from the bottom, taking care that the water should be on the same level in both vessels. As soon as I had adjusted the two thermometers, as in the former experiment, I observed that the top of the fluid was still at 53°, but the bottom had fallen to 49°.

	Top.		Bottom.
In 9 minutes	52°	-	45
— 15 ———	52	-	44

Now, to accelerate the cooling, I withdrew by a syphon the water from the large cylinder, and supplied its place by ice-cold water, mixed with fragments of ice, which by repeated cautious agitation was kept uniformly at the temperature of 32°.

In 23 minutes	48°	-	42+
— 38 ———	44	-	40
— 43 ———	42	-	40
— 46 ———	40	-	40
— 52 ———	36	-	40
— 58 ———	35—	-	39
— 65 ———	34	-	37
— 75 ———	34	-	36
—103 ———	34	-	34

This experiment is the counterpart of the foregoing, and from the testimony of the same instruments, it appears, that when a cylinder of water of 53° is cooled by circumfluent iced fluid, the colder part of the water takes possession of the bottom of the vessel, so as to establish a difference of temperature from the surface, amounting sometimes to 8°. And that as soon as the fluid at the bottom arrives at the 40th degree, the temperature of the fluid in that situation is stationary till the surface reaches the same point.

During the subsequent refrigeration, the progress of the cooling undergoes a total change. The thermometers tell that the colder fluid rises to the surface; so that the top

gets the start of the bottom soon by 4°, and attains the lowest temperature of 34° very long before the other falls to the same degree.

These circumstances, I think, lead to the conclusion, that by the loss of caloric, water at 53° is contracted and rendered specifically heavier, and that this continues to happen till the water come to the temperature of 40°, at which period an opposite effect is produced; for now the water, as it cools, becomes specifically lighter, or is expanded.

In this, as well as the former experiment, the complete change in the situation, which the warmer and colder parts of the fluid affected, in the progress both of the heating and cooling, while every external circumstance of the process continued unaltered, is particularly worthy of remark.

Experiment III.

I took a glass jar, 17·8 inches deep, and 4·5 in diameter, internal measure, having a neck and tubulature very near the bottom. I provided also a cylindrical bason of tinned iron, 4·8 inches deep, and 10 inches in diameter, with a circular hole in the middle of the bottom, large enough to receive the top of the jar. By means of a collar and cement I secured this bason, so that it encircled the upper part of the jar.

The object of the contrivance was to have the means of applying a cooling medium to the superior portion of a cylinder of water, and it answered the purpose completely. I introduced the ball of a thermometer through the tubulature, till the extremity of it nearly reached the axis at three-fourths of an inch above the rising of the bottom, and having fixed it in this situation, I rendered the aperture water-tight, by a perforated cork and lute.

This very tall jar was placed on a table, with the interposition of some folds of thick paper, in a room without a fire, of the temperature 42°.

I filled it with water of 50°, and poured into the bason, which embraced the top, a mixture of powdered ice and salt.

From time to time I explored the temperature near the surface, by inserting the bulb of a thermometer to the depth of half an inch nearly in the axis.

	Bottom.	Top.	Air.
One o'clock	50°	50°	42°
In 11 minutes	46·+	—	
— 15 ———	45	48	

In

	Bottom.	Top.	
In 21 minutes	44 —	46 —	
— 31 ———	42	44	
— 41 ———	41	42	
— 51 ———	40· +	34	At this time a thin film of ice began to form in contact with the glass.
— 1 hour 6 min.	40	34	
— —— 20 ——	39·5		
— —— 44 ——	39·5		
— 4½ hours	39·5		A crust of ice of some thickness now lined the glass, and air had fallen to 40°.
— 5½ ——	39		
— 11 ——, *i.e.* at midnight	39		Crust of ice complete.
— 19 ——, *i.e.* next morning	39		Air 40°.
— 26 ——	40·		Air 40°. So much ice had melted that the cake was detached from the side of the vessel, and floated.
— 32 ——	40		
— 41 ——	40		Air 41°. Ice not all melted.
— 50 ——	41		Air 42°. Ice not entirely gone.

This long protracted experiment presents some striking facts, and its general import, with regard to the subject of investigation, agrees with the preceding. In it we see, that when the frigorific mixture abstracted caloric from the upper extremity of a cylinder of water, nearly 18 inches long, and at 50°, the reduction of temperature appeared sooner, and advanced quicker at its lower extremity than in the axis at the top, not two and a half inches distant from the cooling power. No one can entertain a doubt that this is owing to a current of cooled and condensed fluid descending, and a corresponding one of a warmer temperature ascending. Now, if water observed the same law that other bodies do, and had no peculiarity of constitution, the same progress of cooling should continue. This, however, the experiment teaches us, is not the case: as soon as the fluid at the bottom exhibits a temperature of 40°, it ceases. The colder fluid remains at top, and, quickly losing temperature, ere long begins to freeze. The continuance of the colder fluid at the surface surely denotes, that it is not more dense than the subjacent warmer water. The legitimate inference from this is, that water of temperature 40° is not contracted by being cooled to 32°.

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Did water observe the usual law, and lose volume along with temperature, this experiment, by its long duration, afforded ample time for the manifestation of it.

For not less than two days did ice-cold water maintain possession of the top, and for the same period the temperature at the bottom never fell below 39°. No current, therefore, of cold and condensed fluid moved from the surface, to affect the inferior thermometer, or to attest the contraction of water by cold.

This experiment, however, I must remark, does not warrant the conclusion, that the water is actually expanded, though it in no degree opposes it. It proves no more, than that the contraction ceases at 40°; and that water of 32° is not more dense than of 39° or 40°. Nay, some may perchance allege, that it does not prove so much; conceiving, that if at 40° the contraction, without ceasing altogether, becomes very inconsiderable, the difference of density occasioned by the subsequent reduction of temperature may be so very trifling, as not to enable the cold particles to take that situation which their gravity assigns to them, in opposition to the inertia and tenacity of the subjacent mass; and therefore that the colder, though heavier fluid, may be constrained to remain above. That this allegation should have no weight attached to it, the circumstances of the succeeding experiment will clearly show, as I shall soon notice.

Before quitting the consideration of the present experiment, it may be worth while to remark, that it may seem rather surprising, that the bottom of the fluid was not apparently affected in its temperature by the ice which so long occupied its surface. It might be expected, though no cold currents descended from above, that the caloric should be conducted from below, and that the temperature should by that have been reduced*. I suppose that the caloric did

* This experiment may perhaps be thought to give countenance to the opinion of the very ingenious Count Rumford, that fluids cannot conduct heat, and that no interchange of heat can take place between the particles of bodies in a fluid state, seeing that for two days the fluid at the bottom of the vessel never fell below 39°, though the surface was at 32°.

From the circumstances detailed in his seventh essay, the Count concluded, that heat cannot descend in a fluid. From the present, it might with equal justice be inferred, that heat cannot ascend.

Had I not the fullest conviction that this celebrated philosopher has pushed his ideas too far, I might be disposed to consider this experiment as according well with the hypothesis.

Soon after the interesting speculations of the Count appeared, I began to investigate the subject; and, by a pretty long train of experiments, which I have annually taken an opportunity of detailing in my lectures, satisfied myself

did pass from the lower strata upwards, but extremely slowly, by reason that fluids, as Count Rumford taught us,

myself that he assigned to fluidity a character that does not belong to it. Though since the date of these experiments, the public has become possessed of several series, well devised, and, in my opinion, of themselves conclusive, it may yet be worth while to state the tenor and result of them, by which the value of their testimony in favour of the conducting power of liquids may be estimated.

The experiments were of two descriptions.

The one set, of the same nature nearly with those of Count Rumford, was designed to examine, Whether heat, when applied to the surface, can descend in a fluid; and the other, to discover, Whether, on the mixture of different portions of fluid at different temperatures, an interchange of caloric takes place between the particles;—Water, oil, and mercury, having been the subjects of the Count's experiments, were employed for the first set.

To explore the conducting power of water and oil, the apparatus which I used consisted of two vessels of tinned iron, both cylindrical, and the one somewhat larger than the other. The larger had a diameter of eleven inches, and into it were poured the subjects of the trial, to different depths on different occasions. The smaller was ten and a half inches in diameter. By three hooks it was suspended within the larger pan, in such a manner that the bottom of it exactly reached and came in contact with the surface of the fluid. This smaller vessel became the source of the heat, by being filled with boiling hot water. The water was changed frequently, care being taken to avoid, by the use of a syphon, all agitation and disturbance.

In experiments of this nature, the difficulty is to prevent the conveyance of caloric by the sides of the vessel. I attempted, and, I think, I succeeded, in overcoming this difficulty, by encircling the larger vessel, at a height exactly corresponding with that of the surface of the fluid within, with a gutter or channel about half an inch in depth; and by causing a stream of cold water to flow constantly through a syphon into this gutter, while from the opposite side it ran off by a small spout.

The water was several degrees colder than the subject of the experiment; and keeping cool the portion of the vessel with which it was in contact, it intercepted the heat that would otherwise have travelled by this route to the bottom.

For mercury I had recourse to vessels of glass.

In all the experiments a thermometer bore testimony that the caloric descended from the surface to the bottom of the fluid, and demonstrated, at least to my conviction, that fluids can conduct heat.

The progress of the heat, however, was very slow, and attested the important fact, for which we ought to be thankful to the Count,—That fluids are very bad conductors.

The second set of experiments was calculated to examine, in a very different manner, the position, That all interchange and communication of heat between the particles of fluids is impossible.

When a hot and a cold fluid are mixed together and well agitated, very soon an uniform temperature is produced. This equality must proceed either from a communication of heat from the warmer to the colder fluid, agreeably to the common opinion, or from a perfect intermixture of hot and cold particles, according to the notion of Count Rumford. To which cause it ought to be attributed, I conceived I might discover, by ascertaining whether, after such an intermixture, any separation of the hot and cold portions took place. If the equilibrium of temperature be owing to intermixture without interchange of caloric, the hotter particles, as soon as the agitation ceases, ought, by reason of their greater rarity, to accumulate to a certain degree, at the surface, and there exhibit a temperature above the common one.

 I first

us, are excessively bad conductors of heat, and so very slowly, that the caloric entered from the atmosphere with sufficient quickness to prevent any depression of temperature below the 39th degree.

This experiment, I may conclude with remarking, is very well calculated to exhibit the error of the popular opinion, that "heat has a tendency to ascend."

Experiment IV.

I took the same tall jar, and stopping the tubulature with a cork, I filled it with water of temperature 40°, and placed it in a pan. After suspending two thermometers, as in experiment first and second, I poured a mixture of ice and salt into the pan, to the depth of 4·2 inches, the air of the room being 40°, as in the last experiment.

I first tried water, and mixing this fluid boiling hot, with an equal quantity nearly ice-cold, in a stoppered glass jar, I shook them well for a short time.

I then noticed the resulting temperature, and raising the ball of the thermometer towards the surface, I had an opportunity of observing, that it never rose the smallest portion of a degree above the common temperature which had been established.

I next made a similar experiment with alcohol, selecting it on account of its remarkable dilatability. I shook well, for half a minute, a mixture of equal parts of alcohol at temperature 40° and at temperature 170°. The resulting temperature of the mass was 104°.

Now, if this was a mixture of particles at 40° and at 170°, as the difference of specific gravity between the fluid at these temperatures is very considerable, some separation of the warmer and lighter particles from the others, ought, I conceive, to have taken place. The temperature of the top, however, never indicated the arrival of warmer particles. It never ascended above the point of equilibrium.

From these experiments I concluded, that the uniformity of temperature was established by an actual communication and interchange of heat between the particles.

It may not, however, be improper to state, that Count Rumford, with whom several years ago I had the pleasure of conversing upon this subject, alleged, that the intermixture might be so complete as to prevent any separation whatever.

If it be a property essential to fluidity, that heat cannot pass from one particle to another, the particles of different fluids ought to be equally incapable of imparting caloric mutually to each other. Unfortunately, however, for the speculation, the caloric is so communicated. Though, *à priori*, I entertained no doubts respecting the result of the experiment, I poured a quantity of olive oil which had been heated by immersion in a vessel of boiling water for half an hour, upon an equal volume of water of 38°, and agitated the mixture, by shaking for a quarter of a minute. The common temperature produced was 78°, and the heat had gone from the oil into the water; for when the fluids separated, and had arranged themselves according to their specific gravity, both of them had the same temperature of 78°.

The experiments of the two descriptions now recorded, left on my mind little doubt that the Count had overstrained his conclusions.

Eleven

	Bottom.	Top.	Air.
Eleven o'clock	40°	40°	40°
In 10 minutes	38+	38+	
— 20 ———	38—	38—	
— 30 ———	37—	37—	
— 40 ———	36	36	
— 60 ———	35·5	35·5	
— 80 ———	35	35	
- 100 ———	34·5	35	
- 120 ———	34—	34	
— 8 hours	34—	34	

A crust of ice began to form on the inside of the glass when the water in the axis of the bottom and of the top was at 36°. In the course of the experiment, it became at least an inch thick.

We learn from this experiment, that cold applied to the lower part of a cylinder of water, nearly 18 inches long, and having the temperature of 40°, is actually as speedily perceived at the summit as in the axis of that part, on the external surface of which it immediately acts. As fluids conduct heat so very tardily, this can only arise from currents of cooled water ascending from the bottom, and these cold currents could not move upwards, were not the water of them specifically lighter than that of the incumbent warmer fluid.

The water, therefore, which at the bottom is cooled by the contiguous frigorific mixture, must be expanded by the loss of caloric.

This experiment secures full force to the last, as it obviates the objection already noticed, and also precludes another. I have already stated, that it may perhaps be alleged, that the fluid at the top, in experiment third, though cooled to 32° did not descend, because below 40° the contraction is so trifling, that it does not occasion a difference of specific gravity sufficiently great to cause the particles to descend, when opposed by the inertia and tenacity of the fluid through which they have to fall; or it may be conceived, that the descent is so tardy, that time is given to the ambient air or subjacent fluid to furnish heat enough to raise the temperature of the descending stream, and by that arrest it in its downward course.

But from the particulars above recorded, it is manifest, that the change of density between the temperature of 32° and 40° is quite sufficient to put into motion the particles, and to enable them to overcome the obstacle arising from inertia and tenacity, and to withstand the arresting effects of atmospheric heat.

Though these experiments, and some others of a similar nature, carried conviction to my mind, and perfectly satisfied me respecting the reality of the anomaly of water, I determined to vary somewhat the mode of making the experiment, so as to obtain still more striking results.

For the fifth experiment, I used an apparatus which consisted of a still taller jar. It was 21 inches high, and 4 in diameter. I adjusted at the middle of its height a perforated bason of tinned iron, 2 inches in depth, and 10 in diameter. As this bason embraced the middle of the jar, I could, by filling it with hot water, or a frigorific mixture, apply heat or cold to the middle portion of the fluid in the jar, and thence, by the thermometer, learn what course the heated or cooled fluid should take.

Experiment V.

I filled the jar with water at 32°. I placed it upon several folds of thick carpet, previously cooled to the same degree. The air of the room going from 33° to 35°, I introduced two thermometers, as in experiments first and second. I then poured water of temperature 68° into the bason, and by means of a spout arising from the side of it, and a syphon connected with a reservoir of water at the temperature now mentioned, I renewed the contents of the bason frequently, but without causing any agitation.

	Bottom.	Top.	Air.
At commencement	32°	32°	33—35
In 10 minutes	35	32	
— 15 ———	36—	32	
— 20 ———	36+	32	
— 25 ———	37	33	
— 30 ———	38	33	*** From this time I charged the bason with water of temperature 88°, and renewed it frequently.
— 38 ———	38+	33	
— 45 ———	39—	33	
— 50 ———	39+	44	
— 55 ———	39+	45	
— 60 ———	39+	48	

Nothing can be more decisive with regard to the question in dispute, than the particulars of this experiment. Heat is applied to the middle of a column of ice-cold water. The heated portion has an equal share of the column of cold fluid above it and beneath it. There is nothing to determine its course in one direction or another, excepting its actual change of density.

The thermometer evinces that the warm current sets downwards, and carries the increased temperature to the bottom. There, this instrument indicates the successive

rise

rise of several degrees, before the surface indicates the smallest acquisition of heat.

The inference is plain, that the cold water is contracted by the heat.

The change of the effect of heat is equally well illustrated by this experiment.

No sooner did the inferior portion attain the temperature of 39°, than the heated fluid altered its course, and, by ascending, carried the increase of temperature very rapidly to the surface, so that it soon surpassed the bottom, and continued to rise, while the other remained stationary.

Experiment VI.

I filled the jar used in the last experiment with water of temperature 39½°, the air and the support being at 39°. Disposing the thermometers in the usual manner, I introduced a mixture of snow and salt into the bason.

	Bottom.	Top.	Air.
At commencement	39·5	39·5	39°
In 10 minutes	39+	38+	
— 25 ———	39+	36·5	*** At this time ice began to be formed on the side of the vessel.
— 35 ———	39	36—	
— 55 ———	39	35	
— an hour and 10 min.	39—	34+	
— ——— 35 ———	39—	34—	
— 2 hours	39—	33+	

This experiment speaks in as decided language as the preceding. It shows that when a portion, in the middle of a column of water at temperature 39·5 is cooled, the colder fluid rises, and does not descend through the warmer mass, and presents the unequivocal demonstration, that water of temperature 39½° is actually expanded by losing heat.

The different experiments which I have in detail recorded, agree perfectly with each other in the evidence they give relative to the subject of inquiry. The general import of them is, that water which is ice-cold, or a few degrees warmer, when heated, becomes specifically heavier,—that water of 40° when heated becomes specifically lighter,—that water above 40°, by the loss of heat, or by cold, is rendered specifically heavier; and that water below 40° is, by the same cause, rendered specifically lighter.

Such being the general import, the conclusion is irresistible, that heat, in low temperatures, causes water to contract, and at superior temperatures to expand. The opinion, therefore, is founded in truth, that water possesses

sesses a peculiarity of constitution in relation to the effects of caloric, and that it is, within a short range of temperature, an exception to the general law of "expansion by heat."

So far as I can judge from these experiments, I am disposed to believe that the point at which the change in the constitution of this fluid in relation to heat takes place, lies between the $39\frac{1}{2}^{\circ}$ and the 40th degree.

I am not at present aware of any objection to the method I have followed in establishing this singular anomaly, and in removing any doubts which may have arisen from the unavoidable influence which the instrument must have in the mode of conducting the investigation that had previously been adopted.

The plan of operation above described, however, only ascertains the fact; it gives no data for ascertaining the amount of the anomalous effect of heat.

I have already stated, that M. de Luc alleged, that from the temperature of 41°, the expansion occasioned by cold was very nearly equal to that produced by the same number of degrees of heat; and consequently that water possesses the same density at any given number of degrees of temperature above and below 41°. The first experiments of Mr. Dalton appeared to confirm this opinion, and to enlarge the range to which it applied, by extending it to temperatures as far below 32°, as water allows itself to be cooled before it begins to freeze. From one circumstance that constantly occurred, I am inclined to think, that the amount of the dilatation by cold is inferior to that caused by heat.

During the heating or cooling of water below 40°, the difference of temperature between the top and bottom of the fluid was less than what occurred during the cooling or heating of the fluid through the same number of degrees above it; and I conceive that, when other circumstances, but particularly the rate of the change, are alike, the difference of temperature between the upper and lower parts of the fluid, as it depends upon, may prove a measure of, the difference of density.

Alcohol, when heated or cooled, presents, by reason of its greater expansibility, a greater difference of temperature in these situations than water; and upon the same principle I infer, that water from 40° is more expanded by an equal number of degrees of elevation than of depression.

As the concurrence of the testimony of the experiments above related with the general opinion, will probably remove

move every doubt respecting the matter of fact, it remains a very difficult problem for those who are fond of philosophical investigation, to explain how heat shall occasion in the same fluid, without producing any alteration of mechanical form or chemical condition, at one time contraction and at another expansion, and to reconcile the contractive effect to the conceived notions of the mechanism of the operations of this energetic agent.

When heat causes expansion, it is imagined to act by inducing a repulsion among the particles of bodies, which, opposing and overpowering the cohesive attraction, causes the particles to recede.

In what manner, then, the addition of heat can occasion, or allow, the particles of water to approach each other, and how the subtraction of it can make them retire to a greater distance, I confess I can in no measure comprehend.

An explanation, abundantly plausible at first view, very readily suggests itself to every one who is aware of the great and forcible expansion which happens to this fluid at the moment of its congelation. It is stated by sir Charles Blagden, in the paper already quoted.

The remarkable dilatation which water experiences at the instant of being converted into ice, is very generally ascribed, and I presume very properly, to a new arrangement which the particles assume, determined probably by their polarity; by which one side of the particle A is attractive of one side of B, while it is repulsive of another.

Now, if this polarity operates with so much energy as to impart almost irresistible expansive force at temperature 32°, it is reasonable to suppose that it may begin to exert its influence, though in a far inferior degree, at temperatures somewhat more elevated. The expansion, therefore, that takes place, during the falling of temperature from 40°, may be imputed to the particles beginning or affecting to assume that new arrangement which their polarity assigns them, in which arrangement these particles occupy more space than before.

Again, when heat causes water of 32° to contract, upon the same principle it may be conceived to operate by counteracting the small portion of the disposition to polarity that survives the liquefaction.

I am afraid that we cannot rest satisfied with this explanation. We must not be deceived by the plausibility of it.

The state of perfect fluidity depends upon the circumstance,

stance, that the particles of any body admit of ready motion upon each other, and that the change of relative situation meets with little or no sensible resistance.

Water certainly possesses fluidity in a great degree, and its particles must of course encounter but little resistance, as they glide the one upon the other. But if these particles shall begin to exert any degree of polarity, by which certain faces become more disposed to attach to each other than certain others, this tendency would necessarily oppose that indifference with regard to position, which is essential to fluidity, and of course must impair the fluidity, and induce some degree of tenacity or viscidity.

To appearance, however, water at 32° has its fluidity as perfect as at temperatures considerably elevated. Unwilling to trust to appearance, where experiment might decide, I have attempted in various ways to ascertain whether the water suffers any sensible diminution in this respect while it is expanded by cold. The following method I deem the most correct.

For the purpose, I employed a gravimeter, the one contrived by Mr. Nicholson for discovering the weight and specific gravity of solids.

This is a convenient instrument, but, unfortunately, it is by no means so ticklish as a balance. Duly loaded, so as to be equiponderant with the water in which it is plunged, Mr. Nicholson says, it is sensible to the 20th part of a grain. The one I have, though its stem be slender, is scarcely sensible to less than two or three twentieths of a grain.

The want of sensibility in the gravimeter arises, in a great measure, though not entirely, from a certain degree of tenacity subsisting among the particles of the fluid; and any thing that tends to increase this tenacity, must, in the same proportion, augment this want of sensibility.

To ascertain whether any sensible change in the tenacity or fluidity accompanies the expansion of water by cold, which the theory requires, I examined the mobility of the instrument when immersed in water at different temperatures. I first plunged it into this fluid, heated to between 60° and 70°. Under due loading, which sunk it to the mark on the stem, it was not sensible to a weight less than two or three twentieths of a grain.

I then tried it in ice-cold water, and found that its sensibility was in no perceptible degree impaired. The coldness of the water, it must be remembered, causes some degree of contraction of the gravimeter. This contraction cannot

cannot fail to render the instrument in some small measure more sensible, and, so far as it goes, to counteract the sluggishness produced by any increased tenacity in the fluid.

But as the body of the instrument is made of glass, the amount of the contraction must be very small, and the change of sensibility arising from it so very trifling, as certainly by no means to obscure such an effect as an increase of tenacity would occasion. I therefore with some confidence conclude, that the fluidity of the water is not sensibly diminished, and consequently that the polarity has not begun to exert any sensible influence; it can scarcely, therefore, be accounted the cause of the dilatation.

XXXVI. *On an artificial Substance which possesses the principal characteristic Properties of Tannin.* By Charles Hatchett, *Esq. F.R.S.**

§ I.

The discovery of the principle on which the effects of tanning essentially depend, may be partly attributed to M. Deyeux, who obtained a substance from galls which he considered as a species of resin †, but which was afterwards proved by M. Seguin to be that which renders the skins of animals insoluble in water and imputrescible, and thus to be the principle by which they are converted into leather‡.

The chief characteristic property of this substance was ascertained by M. Seguin to be that of precipitating gelatine or glue from water in a state of insolubility; and as it was evidently different from any vegetable substance hitherto discovered, he gave it the name of tannin.

This discovery of M. Seguin at once unveiled the theory of the art; an easy and certain method was afforded by which tannin could be detected, and its relative quantity in different substances be determined, whilst the nature and properties of this newly discovered vegetable principle could be subjected to accurate investigation.

The former has derived elucidation from the experiments of Mr. Biggin §, and much has been contributed in every respect by M. Proust ||; but the subject has received the

* From the *Transactions of the Royal Society for* 1805.

† *Mémoire sur la Noix de Galle*, par M. Deyeux; *Annales de Chimie*, tome xvii. p. 23.

‡ Ibid. tome xx. p. 15.

§ Phil. Trans. 1799, p. 259.

|| *Annales de Chimie*, tome xxv. p. 225. Ibid. tome xli. p. 331. Ibid. xlii. p. 89.

greatest extension and some of the most valuable additions from the ingenious labours of Mr. Davy, particularly the discovery of the important fact, that catechu or terra japonica consists principally of tannin *.

The united results of the experiments made by these and other eminent chemists, appear therefore to have fully established that tannin is a peculiar substance or principle which is naturally formed, and exists in a great number of vegetable bodies, such as oak-bark, galls, sumach, catechu, &c. &c. commonly accompanied by extract, gallic acid, and mucilage.

But no one has hitherto supposed that it could be produced by art, unless a fact noticed by M. Chenevix may be considered as some indication of it.

M. Chevenix observed that a decoction of coffee-berries did not precipitate gelatine unless they had been previously roasted; so that tannin had in this case either been formed or had been developed from the other vegetable principles by the effects of heat †.

Some recent experiments have however convinced me that a substance possessing the chief characteristic properties of tannin may be formed by very simple means, not only from vegetable, but even from mineral and animal substances.

§ II.

In the course of my experiments on lac, and on some of the resins, I had occasion to notice the powerful effects produced on them by nitric acid; and I have since observed, that by long digestion, almost every species of resin is dissolved, and is so completely changed, that water does not cause any precipitation, and that by evaporation a deep yellow viscid substance is obtained, which is equally soluble in water and in alcohol, so that the resinous characters are obliterated.

When I afterwards had discovered a natural substance, which was composed partly of a resin similar to that of recent vegetables, and partly of asphaltum ‡, I was induced to extend the experiments already mentioned to the bitumens, in the hope of obtaining some characteristic properties by which the probable original identity of these bodies with vegetable substances might be further corroborated. In this respect I succeeded, in some measure, better than I expected; but I observed a very material difference between

* Phil. Trans. 1803. p 233.
† Nicholson's Journal for 1802, vol. ii. p. 114.
‡ Phil. Trans. 1804. p. 385.

3 the

the solutions of the resins and those of many of the bitumens, such, for instance, as asphaltum and jet. The first effect of nitric acid, during long digestion with these substances, was to form a very dark brown solution, whilst a deep yellow or orange coloured mass was separated, which by subsequent digestion in another portion of nitric acid was completely dissolved, and by evaporation was converted into a yellow viscid substance, equally soluble in water and in alcohol, so as to perfectly resemble that which by similar means had been obtained from the resins, excepting that when burned it emitted an odour somewhat resembling that of the fat oils.

It therefore appeared to me that the first or dark brown solution had been formed by the action of the nitric acid on the *uncombined* carbonaceous part of the bitumens, or that by which they are rendered black, and that the deep yellow portion which was separated was that which constituted the real or essential part of these bituminous substances. This opinion was confirmed by some experiments which I purposely made upon amber; and having every reason therefore to believe that the dark brown solution obtained from asphaltum and jet was in fact a solution of coal, I repeated the experiments on several varieties of the pit or mineral coal, from all which I obtained the dark brown solution in great abundance; but those coals which contained little or no bitumen did not yield the deep yellow substance which has been mentioned.

In each experiment I employed 100 grains of the coal, which I digested in an open matrass with one ounce of nitric acid diluted with two ounces of water. (The specific gravity of the acid was 1·40.)

After the vessel had been placed in a sand-bath, and as soon as it became warm, a considerable effervescence, attended with much nitrous gas, was produced: after about two days I commonly added a second and sometimes a third ounce of the acid, and continued the digestion during five or six days, or till the whole, or nearly the whole, was dissolved, excepting in those cases when the deep yellow substance was formed; for this I constantly separated.

The next experiment was made upon charcoal, which was more readily dissolved than the preceding substances, without leaving any residuum; the solution was perfect, and the colour was reddish-brown*.

Having

* The solubility of charcoal in nitric acid, and some of its properties when thus dissolved, have been noticed by professor Lichtenstein in Crell's Chemical

Having thus by means of nitric acid obtained solutions from asphaltum, from jet, from several of the pit-coals, and from charcoal, I evaporated them to dryness in separate vessels, taking care in the latter part of the process to evaporate very gradually, so as completely to expel the remainder of the acid without burning the residuum; this, in every case, proved to be a brown glossy substance, which exhibited a resinous fracture.

The chemical properties of these residua were as follows.

1. They were speedily dissolved by cold water and by alcohol.

2. Their flavour was highly astringent.

3. Exposed to heat, they smoked but little, swelled much, and afforded a bulky coal.

4. Their solutions in water reddened litmus-paper.

5. The same solutions copiously precipitated the metallic salts, especially muriate of tin, acetite of lead, and oxysulphate of iron. The colour of these precipitates was commonly brown, inclining to that of chocolate, excepting the tin, which was blackish-gray.

6. They precipitated gold from its solution, in the metallic state.

7. They also precipitated the earthy salts, such as the nitrates of lime, barytes, &c. &c.

8. The fixed alkalis, as well as ammonia, when first added to these solutions, only deepened the colour, but after some hours rendered them turbid.

9. Glue or isinglass was immediately precipitated by these solutions from water, and the precipitates were more or less brown according to the strength of the solutions. The precipitates were also insoluble in cold and in boiling water, so that in their essential properties they proved similar to those formed by the varieties of tannin hitherto known, with the difference, that this factitious substance appeared to be exempt from gallic acid and mucilage, which commonly accompany the varieties of tannin, and which occasion modifications in the colour and appearance of some of their precipitates.

Having thus had the satisfaction to discover that a product resembling tannin could be formed by such a simple method, not only from vegetable, but also from mineral coal, I was induced to examine how far the same might be extended to animal coal, and I therefore reduced a portion of

Chemical Annals, 1786; by Mr. Lowitz (Crell's Chemical Journal, translated into English, vol. ii. p. 255.); and by Mr. Jameson, in his Outline of the Mineralogy of the Shetland Islands, &c. 8vo. edit. p. 167.

of isinglass to that state in a close vessel, and having rubbed it into fine powder, I digested it with nitric acid in the manner which has been described. At first the acid did not appear to act upon it, but at length it was slowly dissolved excepting a small quantity, which however was in every respect unchanged; and here we may remark, that as animal coal is incinerated with much more difficulty than vegetable coal or charcoal, so was the same difference to be observed when oxygen was presented to these bodies in the humid way.

The solution resembled those which have been described, excepting that the brown colour was more intense. It was evaporated to dryness, and was then dissolved in distilled water, after which the solution, being examined by the reagents which had been employed in the former experiments, was found to produce similar effects, excepting some difference in the colour of the precipitates.

I next added some of the liquid to a solution of isinglass, and obtained a copious precipitate. Thus it is evident, that a tanning substance may be formed from animal as well as from vegetable and mineral coal; and it is not a little curious, that this enables us to assert as a matter of fact, although not of œconomy, that one portion of the skin of an animal may be employed to convert the other into leather.

In the course of these experiments I also subjected coak to the action of nitric acid, and obtained a product which resembled that which had been afforded by pit-coal; but in this case (as might be expected) there was not any appearance of the deep yellow substance which has so often been mentioned.

These experiments, therefore, prove, that a tanning substance may be artificially formed by exposing carbon to the action of nitric acid; and it also appears, that this is best effected when the carbon is uncombined with any other substance excepting oxygen. The following experiments seem to corroborate this opinion.

1. A piece of Bovey coal, which had perfectly the appearance of half-charred wood, was reduced to powder, and was digested with nitric acid until the whole was dissolved.

The colour of the solution was deep yellow; and, by evaporation, a yellow viscid mass was obtained, which was dissolved in distilled water. This solution was then examined by various reagents, and particularly by gelatine, but not any vestige of tanning matter could be discovered, and the predominant substance appeared to be oxalic acid.

 2. Another

2. Another piece of Bovey coal, which had less of the characters of wood, and was more perfectly carbonized, was treated in the way which has been described; the solution was brown, and, unlike the former, afforded a considerable precipitate with gelatine.

3. A portion of the first sort of Bovey coal was exposed to a red heat in a close vessel, and was then reduced to powder and digested with nitric acid; here a remarkable difference was to be observed, for nearly the whole was thus converted into the tanning substance.

4. A coal from Sussex, extremely like the second sort of Bovey coal, also afforded the same product.

5. A piece of the Surturbrand from Iceland yielded a similar result.

6. Some deal saw-dust was digested with the nitric acid until it was completely dissolved; by evaporation a yellow viscid mass was obtained, the solution of which in water afforded results like those of the first experiment on the Bovey coal, for oxalic acid was found in it, but not any of the tanning substance.

7. Another portion of the same deal saw-dust was converted into charcoal in a close vessel; the charcoal was then treated in the manner already described, and was thereby formed into a liquid which copiously precipitated gelatine.

8. Having previously ascertained that teak wood does not contain gallic acid nor tannin, I reduced some of it into charcoal, which was afterwards readily converted into the substance abovementioned.

In these experiments, the deal and the teak wood had been reduced to the state of coal, as usual, by fire; but as this does not appear to have been the means generally employed by nature to convert organized substances into the varieties of mineral coal, I for a considerable time previous to the discovery of the artificial tanning product had been employed in a series of experiments on the slow carbonization of a great number of vegetable substances by the humid way.

The agent which I most commonly used to produce this effect was sulphuric acid occasionally diluted; and although many of the processes were extremely unpleasant and tedious, yet I have not any reason to regret the time which has been thus employed. The subject, however, I foresee will branch out into several details, none of which as yet I can regard as sufficiently completed to merit the honour of being submitted to this learned society; but I am in a manner almost compelled in the present case to anticipate a few

a few of the experiments, with their results, because they are intimately connected with the subject now under consideration.

In these experiments I have observed that concentrated sulphuric acid, when poured upon any of the resinous substances reduced to powder, dissolved them in a few minutes; at this period the solution was transparent, commonly of a yellowish-brown colour, and of the consistency of a viscid oil. But if, after this, the vessel was placed on a sand-bath, the colour of the solution became progressively darker, sulphureous gas was evolved, and at length the whole appeared like a very thick liquid of an intense black. I purposely for the present pass over many phænomena, some of which are peculiar to the different substances when thus treated, whilst others are general, and may be referred to those attendant on etherification, for my intention here is only to notice, in a concise manner, such as immediately tend to elucidate the subject of this paper.

When concentrated sulphuric acid is poured on the common turpentine of the shops, it almost immediately dissolves it like the solid resins; and if a portion of this solution be poured into cold water, the turpentine is precipitated in the solid brittle state of common yellow resin. But if a second portion of the same solution, after the lapse of an hour or more, be in like manner poured into cold water, the resin thus formed is not yellow, but dark brown; and if four or five hours are suffered to elapse before a third portion is poured into water, the resin is found to be completely black. After this, supposing the digestion to be carried on during several days, or until there is no longer any production of sulphureous gas, the turpentine or resin will be found converted into a black porous coal, which, if the operation has been properly conducted, does not contain any resin, although a substance may frequently be separated by digestion in alcohol, which I shall soon have occasion to notice.

When common resin was thus treated, I obtained about 43 per cent. of the coal, which, after being exposed to a red heat in a loosely covered platina crucible, still amounted to 30 per cent., and by the slowness of its combustion and other circumstances which need not here be related, approached very nearly to the characters of some of the mineral coals*.

The

* The difference of the quantity of carbon, which may be obtained in the state of coal from resinous substances by the humid and by the dry way,

is

The effects produced by sulphuric acid upon turpentine and resin are manifestly caused by the union of the two constituent principles of the latter (namely, hydrogen and carbon) with part of the oxygen of the former, so that sulphureous acid, water, and coal are produced. I therefore availed myself of this process, by which coal could be progressively formed, whilst the original substance was gradually decomposed, to make the following experiment.

A quantity of common turpentine was treated with sulphuric acid in the way which has been described, and different portions of the solution being poured at different periods into water whilst the remainder was digested during several days, I thus obtained, from the same original substance, yellow resin, brown resin, black resin, and coal. I then digested a portion of each of these, as well as some of the turpentine, in separate vessels with nitric acid until they were completely dissolved, and afterwards reduced them to dryness. The different residua varied in colour from yellow to dark brown, corresponding to the substances which had been employed. These were then dissolved in distilled water, and were examined by solution of isinglass and other reagents.

1. The solution of the residuum of turpentine was pale straw colour, and did not precipitate gelatine.

2. That of yellow resin resembled the former in every respect.

3. That of the brown resin was of a deeper yellow, but in other particulars resembled the above.

4. That of the black resin, on the contrary, yielded a considerable portion of the tanning substance,—and

5. That of the coal afforded it in great abundance.

Hence it appears, that these different modifications of turpentine yielded the tanning substance only in proportion to the quantity of their original carbon, which, by oxidizement, had been progressively converted into coal*.

Other substances, when reduced into coal in the humid way, were in like manner formed into the tanning substance by nitric acid. In fact I found this to be the constant result, and amongst the many substances which were

is very considerable; we may take common resin as an example, for when 100 grains were exposed to simple distillation in a small glass retort placed over an open charcoal fire, the residuum of coal only amounted to three-fourths of a grain.

* Some late experiments have however convinced me that carbon need not be absolutely converted into coal in order to produce the artificial tanning substance; but this will be more fully explained in a subsequent paper.

examined,

examined, I shall mention various kinds of wood, copal, amber, and wax, all of which, when reduced to coal by sulphuric acid, yielded similar products, by subsequent treatment with nitric acid.

But this substance may likewise be artificially produced without the help of nitric acid, although in a less proportion, as well as with some slight variations in its characteristic properties; for, as I have already observed, when any of the resins or gum resins (such as common resin, elemi, assafœtida, &c.) have been long digested with sulphuric acid so as to assume the appearance and general characters of coal, if afterwards they are digested with alcohol, a portion is dissolved, and a dark brown solution is formed which by evaporation yields a mass soluble in water as well as in alcohol, and which precipitates gelatine, acetite of lead, and muriate of tin, but produces only a very slight effect on oxysulphate of iron. This substance, therefore, which may thus be separated by alcohol from the coal formed from resinous bodies by sulphuric acid, evidently contains some of the tanning matter, which has been produced during the carbonization of those substances.

A natural process very similar to this I much suspect takes place in some cases where peat is formed; I say in some cases, because the production of tanning matter does not seem to be absolutely a necessary consequence attendant on the formation of peat; for in many places where the latter abounds the former cannot be detected, whilst in others it is very abundant, and acts powerfully on animal bodies which have accidentally been exposed to its effects.

There are many facts of this kind upon record, such as the account of the bodies of the man and woman preserved in the moors near the woodlands in Derbyshire, and also of the woman found in the morass at Axholm, in Lincolnshire *. Now I am much inclined to believe, that the tanning substance which so much abounds in these and some other peat moors, did not originally exist in the vegetable substances from which the peat has been produced, but that it has been and continues to be progressively formed (under certain favourable circumstances) during the gradual carbonization and conversion of the vegetable matter into peat.

§ III.

In most of the former papers which I have had the ho-

* Phil. Trans. vol. xxxviii. p. 413. Ibid vol. xliv. p. 571.

nour to lay before the Royal Society, I have, for greater perspicuity, generally concluded with a recapitulation of the contents; but in the present case this appears to be superfluous, as the whole may be concentrated into one simple fact, namely, that a substance very analogous to tannin, which has hitherto been considered as one of the proximate principles of vegetables, may at any time be produced, by exposing carbonaceous substances, whether vegetable, animal, or mineral, to the action of nitric acid.

Since the preceding experiments were made, I have further proved the efficacy of this substance by actual practice, and have converted skin into leather by means of materials which, to professional men, must appear extraordinary, such as deal saw-dust, asphaltum, common turpentine, pit-coal, wax candle, and a piece of the same sort of skin.

Allowing, therefore, that the production of this substance must for the present be principally regarded only as a curious chemical fact not altogether unimportant, yet, as the principle on which it is founded appears to be developed, we may hope that a more œconomical process will be discovered, so that every tanner may be enabled to prepare his leather even from the refuse of his present materials.

The organized bodies and their products have only of late years much attracted the attention of chemists, many of whom, even at this time, (although the modes of chemical examination have been so much improved) seem disgusted and deterred by the Proteus-like changes which take place whenever these substances are subjected to experiment.

But these variable and endless alterations of their properties seem rather calculated to operate as incitements to investigation; for by the accumulation of facts resulting from the changes produced in these bodies by disuniting and by re-combining their elementary principles, not only will chemistry as a science become further illumined and extended, but it will, as it has hitherto done, render great and essential services to the arts and manufactures.

XXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY, LONDON.

The meetings of this society commenced, after the summer vacation, on Thursday, November 7, the hon. Mr. Greville,

Greville, in the chair. The business of the season opened, as usual, with the Croonian lecture, by Mr. Carlisle, *on the Power and particular Structure of the Muscles of Fishes.* The excellence of Mr. Carlisle's former lecture on muscular motion awakened curiosity; and if in the present he has been less successful, it is perhaps rather owing to the more limited nature of his subject, than to the want of original observations and experiments. After several minute physiological explanations of the nature and peculiar structure of the muscles of fishes, and their invariable insertion in fleshy instead of tendinous matter, the author proceeded to detail his experiments on their power and particular use, in enabling the animal to move with so much rapidity through a fluid so dense as water. He ascertained that the muscles of the sides and tail are solely those by means of which the fish advances; that the pectoral and abdominal fins serve only to raise or lower, and balance it in the water; and that, deprived of these muscles and again put into water, it remained at the bottom without being able to ascend, but with the power of advancing as before. By cutting off the pectoral and abdominal fins of one side, the fish lost the power of balancing itself upright, fell on its side, and advanced slowly. He accounted for what is vulgarly called drowning in fishes, when caught by the angler's hook, from the very great violence with which they at that time strike the water, and consequent prostration of strength in the muscles, that they fall on their side or back, and appear as if drowned. The effect would be the same if the hook was placed in any other part of the body as well as the mouth; exhaustion, and not suffocation being the cause.

On the 14th, the reading of this interesting paper was concluded. The fluids of fishes coagulate at about 160° of Fahrenheit.

On the same evening was read the Bakerian lecture *on the Force of Percussion,* by Dr. Wollaston, secretary of the society. The nature of mathematical discussions renders analysis extremely difficult, although Dr. Wollaston's lecture possesses the rare merit of brevity and perspicuity. The hitherto undefined, and perhaps undefinable force of animal power has long exercised the ingenuity of mathematicians, and Dr. Wollaston has wisely contented himself with following Smeaton on that subject. His illustration of the force of percussion, however, and of accelerated motion, he conceives to be somewhat novel, and to contain

something overlooked by his predecessors; it has certainly much simplicity.

SOCIETY OF ANTIQUARIES.

The meetings of this society took place on the same evening and hour immediately preceding that of the Royal. On the first night was exhibited a bronze bas-relief of a boy riding on a dolphin, with a head somewhat depressed resembling that of a bird. This curious and highly interesting piece of antiquity was found at Colchester; but the discoverer could give no satisfactory account of its nature or use. He supposed it to have been one of the *Dii Penates* or *Lares*, and that it represented Cupid, the god of love, in one of the various characters in which the antients adored that powerful deity. Were we permitted an observation different from these learned antiquaries, we would pronounce it not a Cupid but a *Bacchus seated on a dolphin*, the head of which is depressed, indicating the descent of the jolly god *ad inferos*, which, according to the modern system of explaining mythology, was emblematic of the sun's setting apparently in the sea. This figure is supposed to have been adopted by the Greeks from the inhabitants of the north-west, which they called *Skiros*, where their Cimmerian Tartarus was placed. The discovery of this bronze bas-relief will, we hope, lead to more accurate knowledge of the northern mythology.

On the 14th, the several silver coins of Edward III., and two Roman copper coins (one of Claudius) were exhibited. The latter were found in the bed of the Thames, opposite Sion-house, near Kew. Two drawings of paintings, discovered in repairing the walls of St. Stephen's chapel, Westminster, were also displayed. They are about three feet by two, and consist of several persons around a table in one compartment; in the other, of three female figures with an *aureola* indicative of their saintship. Both the male and female countenances have the air of Normans: perhaps the painter may have been of that country.

On the 21st, this society was occupied with the election of a member of its council.

FRENCH NATIONAL INSTITUTE.

The class of physical and mathematical science proposes for the subject of a prize, which it will adjudge in the public sitting of the first Monday of Messidor, in the year 15, the following question, which it remits to the meeting, *viz.*

To determine by observations and by anatomical and chemical

chemical experiments, what are the phænomena of the torpor which certain animals, such as marmots and dormice undergo during winter, with respect to the circulation of their blood, their respiration, and their irritability; also to inquire what are the causes of this sleep, and why it is peculiar to those animals.

It is required that a precise detail be given of the ordinary temperature of these animals, of the degree of cold necessary for their torpor, of the natural temperature which they retain during their lethargy, of the time which is necessary for them to resume their natural temperament when they waken; of the quickness of their pulse during the two states; of the quantity of oxygen which they consume in a given time, both in their quick and torpid state; of the profoundness of their torpor, both with respect to their sensibility and to their simple muscular irritability: finally, of the colour and the chemical state of their blood.

The proposers of the prize do not require that the experiments be made on all dormant animals; only that they be made with strictness on the kinds most easy to be procured.

The prize will be a kilogramme of gold (about 3,400 francs). The memorials sent for competition must be remitted to the secretaryship of the Institute before the first Germinal, of the year 15.

Conditions of the Competition.

All persons excepting the members of the Institute are admitted to the competition.

No work that is sent to the competition is to bear the name of the author, but only a sentence or device.

It is allowed, if the writers choose, to attach a separate and sealed note, which shall inclose, besides the motto or device, the name and address of the author. This note is not to be opened unless the piece obtain the prize. The works destined for competition may be sent to the office of the secretary, franking the packet which contains them. The clerk of the secretaryship will give receipts for them. They may also be addressed, carriage paid, to the perpetual secretaries of the physical and mathematical sciences.

The competitors are informed, that the Institute will not return any of the works which shall have been sent to the competition. The authors shall have liberty to take copies of them, if they have occasion. The administrative commission of the Institute will deliver the gold medal to the bearer of the receipt. In case there shall have been no receipt

ceipt taken, the medal will not be sent, but to the author himself, or to the bearer whom he shall employ.

CELTIC ACADEMY, PARIS.

This academy at a late meeting submitted to the test an ingenious contrivance of one of its members, which communicates the faculty of corresponding and conversing with persons of whose language you are entirely ignorant, without anypreliminary study, without expense, without embarrassment, or the least mental exertions. It was tried by twenty-five academicians on the European languages, and this trial demonstrated, that by means of this discovery, a person may travel wherever he pleases without an interpreter; that he may ask for every thing he wants, converse on every kind of subject interesting to a traveller, and even express metaphysical ideas. This process is intended to be made public.

LITERARY SOCIETY, MANILLA.

Dr. Anderson, of Madras, has published in the Madras Gazette the following letter, which he had lately received from the Manilla, announcing the formation of a literary society in that city:

"There is lately instituted here, under the immediate protection of government, a literary society, to which they have done me the honour to appoint me secretary. The intention of this society is to produce a journal every month, treating of the different branches of useful sciences of the Philippine islands, in order to encourage industry. Each will begin with an historical extract of these islands since the commencement of their establishment by the Spaniards, drawn from the most approved authors on this subject, deprived of all superstition in the antient relations. After that they will speak of the three kingdoms, the animal, the vegetable, and the mineral. Agriculture will occupy a great space; and commerce and industry will furnish the journal with something upon navigation. A few sheets will be reserved for the remarkable events of every description which may have occurred, with observations on their different accidents. This is nearly the plan, which you will be able to judge more of by the prospectus, which I shall have the honour of sending to you by the first opportunity; but it is at present in the press, and will not appear before the end of the month. The society, wishing to acquire all the information and light which can tend to render their work more useful, and at the same time enter into

into a correspondence with the other different societies who are occupied by the same views, have requested me, and in particular the president, don Domingo Goyena, to inform the society at Madras of their intentions by this opportunity, until they can do it more formally by sending the prospectus of their journal. Not knowing any of the other members of this society excepting you, sir, I take the liberty to request you will engage the learned members of your assembly in favour of this infant society—*Friends of Luçonia*—and engage them to admit with benevolence the request to enter into correspondence, and make known to this infant in the cradle their lights, their works; and, in fine, to assist it with their succour, that it may one day be enabled to tread in the steps of its masters. I cannot help being extremely flattered, sir, by a commission which brings to my recollection a person of your merit, and which will often give me the opportunity to assure you of the sentiments of respect and high consideration with which

"I have the honour to be, Sir,

"Your very humble and very obedient servant,

Manilla,
10th Feb. 1804.

"J. M. Dayot."

SOCIETY AT BOMBAY.

A society has been instituted at Bombay for the purpose of collecting useful knowledge in every branch of science, and of promoting the further investigation of the history, literature, arts and manners of the Asiatic nations. Sir James Mackintosh, who was elected president, delivered a very eloquent discourse on the occasion.

XXXVIII. *Intelligence and Miscellaneous Articles.*

VACCINATION IN INDIA.

To the Editor of the Government Gazette.

SIR,

The settlements of Columbo, Madras, and Calcutta, having started nearly at the same time in the race of vaccine inoculation similarly equipped, it may be worthy of inquiry, how so great a difference should exist in their progress as appears by public papers; seeing that some time ago, when the vaccine returns at Madras gave 145,000, those at Calcutta were only 11,000,; and now, by the Ceylon Government Gazette of the 5th instant, 26,000 persons have been vaccinated there notwithstanding the

pressure of war on that island, and on the last returns of the accountant-general here amount to 216,000.

From the difficulties and inconveniences occasioned by war, however, neither the coast nor Bengal have been exempt, and therefore this cannot be held as a sufficient reason of disproportion. The zeal of professional men every where, with very few exceptions, having been nearly the same, the real cause therefore remains a desideratum; but the display of so many operations having lately excited a variety of plans of alterations amongst several gentlemen of this establishment, renders it in some degree proper to pay attention to the idea of a permanent and fixed institution, so that every village may have vaccine inoculators within itself, in the same or a similar manner to such useful avocations as you will see detailed by me, in a letter to captain James Achilles Kirkpatrick, page 89 of the accompanying volume, which you may therefore reprint in the Gazette to-morrow, along with this letter.

I am, Sir,

Your very obedient servant,

J. ANDERSON.

Fort St. George.
Dec. 19, 1804.

To Captain James Achilles Kirkpatrick.

DEAR SIR,

The compass of a letter hardly admitting room for answers to Mr. Morton Pitt's queries, I must again trouble you to render the answer which I have given to his question of "what proportion the rent bears to the year's crop" more intelligible, by stating the practice at the village of Nungambacum, where I am situated, which may serve to give an idea of the distribution of grain, the greatest necessary of life, from the very spot where it grows; a practice that seems favourable to the preservation of good-will between the different ranks of society.

In this village twelve families of labourers have twenty-five ploughs, with which they cultivate 160 cawnies* of ground, paying rent to government, and 40 cawnies of free land; and as the question only relates to the first, it will be sufficient to say that the cawnie generally produces sixty marcals † of paddy, of which, when cut and gathered into a heap, the cultivator must give one marcal to the carpenter and iron-smith, and another to the washerman and barber.

* One cawnie is 57,000 square feet.
† One marcal is eight measures.

A bundle

A bundle of the straw, containing one measure* of paddy†, is folded up and given away, when the crop is divided with government, to Pulliar church, another to the Eesvaren church, Permall church, the Vadum schoolmaster, the water-charity pandall, the village beadle, the amildar office water-woman, the doctor, the taylor, the man who burns the dead and waters the fields.

When the heap is trodden and separated from the straw, the taylor sings a song to render the crop propitious, for which he gets a measure of the unwinnowed grain.

When the crop is properly cleaned, the church of Eesvaren, the church of Permall, the church of Peduareear, the village extra servant, the Vadum schoolmaster, the dancing girls who attend marriages and funerals, and the lamp-oil for the amildar's office, have each two measures.

The village clerk six measures; the watchman, by guess of hand, one measure; Pulliar church, half a measure; the water-charity pandall, the same quantity; as well as the village carpenter, ironsmith, washerman, barber, beadle, water-woman, and doctor.

After the above business is over, the village clerk distributes, to the cultivator, two marcals and seven measures; the head man of the village, one marcal and two measures; the watchman, one marcal; the village servants, altogether six measures; the head bramin, two measures; Caulatapetty church, five measures; Eesvaren church, one measure and a half; Permall church, one and a half; repair of the tank, one marcal and four measures.

The remaining grain is then divided between the cultivator and government, deducting one marcal and a half from government's share for the watchman and clerk's fees.

Fort St. George, May 21, 1798. JAMES ANDERSON.

PRESERVING OF HOPS.

The following method of preserving hops is given in Klaproth's Journal of Chemistry:

The author, from experiments he has made, proposes to distil the hops with water: when the distilled water is drawn off, the essential oil which floats on the surface is to be separated; the refuse which remains in the still is to be well squeezed, and boiled a second time, after which it is to be evaporated to the consistence of extract, adding a little distilled water. When required for use, the essential

* A measure is thirty-four ounces.

† Paddy is rice in the husk.

oil

oil is pounded with a little sugar, then diluted with the extract in some new beer. The beer is rendered somewhat bitter by it, from which the author concludes that one must either save a quantity of hops, or obtain beer which will keep much better, consuming no more hops than usual.

He proposes also, in years when hops are scarce, to substitute for them the *menyanthes trifoliata,* (the common buck-bean, or marsh trefoil, marsh clover) taking care to add a fourth or a third part of hops.

On the face of this process it is not easy to perceive that any thing is gained by this extra labour, which is not secured by boiling the hops in the beer itself as is usually done. The only question is, Whether a larger portion of the useful parts of the hops can, in a plentiful season, be preserved by making the kind of extract recommended, than by keeping the hops themselves. It is certain that much volatile matter is dissipated from hops kept in bags in the usual manner; but probably a better way to prevent this waste would be by packing them up in tight casks, to prevent this evaporation.

ASTRONOMY.

Table of the right Ascension and Declination of Ceres, Pallas, and Juno, for December 1805.

	Ceres.					Pallas.					Juno.				
	A.R.			Dec.N.		A.R.			Dec. S.		A.R.			Dec.S	
1805	h	m	s	°	′	h	m	s	°	′	h	m	s	°	′
Dec. 1	7	45	40	25	54	4	59	24	32	34	11	17	48	1	2
4	7	44	28	26	12	4	56	48	32	51	11	20	48	1	17
7	7	43	0	26	30	4	54	12	33	3	11	23	36	1	30
10	7	41	12	26	48	4	51	32	33	10	11	26	20	1	43
13	7	39	12	27	7	4	48	52	33	11	11	28	52	1	54
16	7	37	0	27	26	4	46	20	33	9	11	31	16	2	4
19	7	34	32	27	45	4	43	52	33	1	11	33	28	2	13
22	7	31	56	28	3	4	41	32	32	49	11	35	28	2	21
25	7	29	4	28	22	4	39	24	32	32	11	37	20	2	27
28	7	26	8	28	40	4	37	24	32	11	11	39	0	2	32
31	7	23	4	28	58	4	35	44	31	45	11	40	28	2	35

Errata.—In the Table given in No. 88, the hours, instead of being continued down the columns, as in the first and second lines, viz. "7" in the one and "5" in the other, were marked "0".

THE

THE IBIS.

In our present number we have given a skeleton of this antient sacred bird of the Egyptians. An engraving of the bird itself, accompanied with a description, was given in our eighth volume.

LIST OF PATENTS.

A grant unto Richard Kentish, late captain in the Cambridgeshire militia, but now of Birmingham, in the county of Warwick, esq.; for his invented armour waistcoat, which is a sure defence against the bayonet, sword, pike, or any pointed instrument, and in many instances may prevent the wound from a musket ball. Dated Oct. 30.

To Joseph Huddart, of Highbury Terrace, in the county of Middlesex; for sundry new improvements in the manufacture of large cables, and cordage in general. Dated October 30.

To Samuel Meller, of Gresse-street, in the county of Middlesex, engineer; for certain improvements on steam engines. Dated October 30.

To John Hartop, of Brightside, in the county of York, iron-master; for certain improvements in the method of preparing malleable iron for the purpose of making the same into bars, sheets, and slit rods, and manufacturing the same also into hoop iron, and that he has invented certain improvements in the method of preparing all other malleable metals. Dated November 7.

To John Trotter, of Soho Square, in the county of Middlesex, esq.; for a rotatory engine for applying the powers of fluids as first movers. Dated November 14.

To William Milton, vicar of Heckfield, in the county of Southampton, master of arts; for a mode of rendering carriages in general, but particularly stage coaches, more safe than at present, and various other improvements upon such carriages. Dated November 16.

To John Curr, of Sheffield Park, in the county of York, gentleman; for a method different from any that has hitherto been invented or known of laying a rope; or, in other words of twisting and forming the strands together that compose the round rope. Dated November 16.

To Andrew Flint, of Gee-street, Goswell-street, in the county of Middlesex, millwright, for a machine upon an improved construction, which may be used as a steam engine. Dated November 16.

METEOROLOGICAL

METEOROLOGICAL TABLE

BY MR. CAREY, OF THE STRAND,

For November 1805.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Oct. 27	50°	50°	44°	29·40	0°	Rain
28	44	46	43	·64	15	Cloudy
29	44	44	35	·55	0	Rain
30	35	48	34	30·00	20	Fair
31	32	41	37	·40	15	Foggy
Nov. 1	36	43	36	·15	27	Cloudy
2	35	47	35	·08	25	Fair
3	32	48	38	·35	15	Fair
4	36	48	38	·32	17	Fair
5	31	38	37	·39	0	Foggy
6	38	43	42	·39	0	Cloudy
7	43	44	40	·38	0	Foggy
8	40	40	39	·36	2	Foggy
9	41	41	39	·20	3	Cloudy
10	40	42	40	·16	5	Cloudy
11	40	42	39	·32	10	Cloudy
12	39	42	39	·30	6	Cloudy
13	42	43	39	·40	18	Cloudy
14	39	44	41	·52	15	Fair
15	41	48	40	·65	18	Cloudy
16	40	46	40	·65	18	Cloudy
17	34	41	39	·55	15	Fair
18	30	42	34	·18	20	Fair
19	31	39	38	·05	0	Foggy
20	38	44	35	·25	10	Fair
21	30	38	35	·36	12	Fair
22	35	42	35	·20	10	Fair
23	34	42	36	·28	18	Fair
24	33	43	40	·25	4	Showery
25	40	44	42	·20	6	Cloudy
26	42	46	41	·20	20	Cloudy

N. B. The barometer's height is taken at noon.

XXXIX. *Examination of different Processes for obtaining the Separation of Nickel from Cobalt.* By CHRISTIAN FREDERIC BUCHOLZ*.

THE want of pure nickel and oxide of cobalt determined me, for the sake of obtaining it, not only to make experiments of my own invention, but also to repeat different processes proposed for the same object. As it happens but too frequently that some accidental circumstances in experiments are omitted by their authors, or that we are unable, from ignorance of certain indicative characters, to employ the most scrupulous exactness in examining the products obtained,—a circumstance which makes the process we follow give evidence of its own insufficiency,—it will be agreeable to chemists to find here an abridged explanation of my experiments on this matter: the results they have yielded me, added to those which the labours of other chemists have afforded them, will point out the most convenient way to the end proposed, while they save unfruitful trouble and expense.

A. The able chemist Hermstadt had proposed for making the separation of oxide of cobalt from the oxide of nickel, a method which consists in dissolving in ammonia the nitrate or the sulphate of cobalt impregnated with nickel, and to expose the solution to a single evaporation. I proposed to try this process with the contrary design.

1. An ounce of ore of earthy cobalt was therefore dissolved by heat in four ounces of nitric acid of the specific gravity of 1·220, in which had been put an equal quantity of water, which gave in residue three gros of oxide of arsenic under the form of little crystals. The solution, diluted with a portion of water, was of a dingey green colour. It was filtered, and diluted with a greater quantity of water, and a little oxide of bismuth was separated. Caustic ammonia was added in excess until it made no more sensible solution in the obtained precipitate. That which was not dissolved, being of a reddish white, was a composite of the arseniate of cobalt with a little oxide of bismuth and oxide of iron. The filtered solution, which was of a beautiful blue, was evaporated in a gentle heat, by which means about two gros of a beautiful clear green matter was deposited, which on examination was found to be an oxide of nickel

* From the *New Universal Journal of Chemistry* of Messrs. Bucholz, Crell, &c., vol. iii.

nickel united with oxide of cobalt. The filtered liquor, having been afterwards evaporated by the heat of a stove, still deposited some oxide of the same quality. The saline mass of nitrate of ammoniated nickel, of a deep green colour, which had been obtained by evaporation, was redissolved, filtered, and kept in ebullition with an excess of caustic potash till the ammonia was completely evaporated. By this means there was still separated a gros and a half of the oxide of nickel, which did not appear to contain any thing more than a little oxide of cobalt.

2. As the separation was not effected very perfectly or easily by the process I have mentioned, I made trial of sulphuric acid. For this end, I poured an equal quantity of water on the oxide formerly obtained, and I added sulphuric acid. By the aid of heat the whole was dissolved. There was then disengaged, in a very evident manner, an odour similar to that of oxygenated muriatic acid, although there had not been an atom of muriatic acid employed. I have before observed the same phænomenon on a similar occasion. The solution was afterwards treated with ammonia, as formerly, until it was almost all redissolved: the residue, which consisted of oxide of cobalt with a little oxide of nickel, had a colour of verdigris. When the solution had been evaporated in a gentle heat, and was separated by filtration from the deposited powder, of which the greater part was oxide of cobalt, it was left to a spontaneous evaporation. It crystallized, without any other separation, in groups of prismatic crystals, partly of a pale green, and in crusts of an azure colour at the edges. A trial of the oxides separated by potash from the solution of these crystals, as well as from the mother liquors, made it obvious that they both contained cobalt nearly in the same proportion.

3. Though in this case the result was unsuccessful, I resolved, nevertheless, to recommence the same experiment on a greater quantity, hoping that the separation might better succeed by crystallization. In consequence, the oxide being separated by the carbonate of potash from a solution of eight ounces of ore of earthy cobalt in common nitric acid (which had been before evaporated and filtered), this oxide, treated with a sufficient excess of potash to separate from it. as much as possible, the arsenic acid which might be combined with it, was dissolved in the sulphuric acid weakened by eight parts of water. Some pure ammonia was added to the liquid, so as to dissolve what was soluble in the precipitate. The filtered solution was evaporated by a moderate ebullition, and afterwards left to spontaneous evaporation.

evaporation. After having parted with a little cobalt mixed with nickel of a pale green, the saline part collected itself, by little and little, into crystals of a blueish green, which could here and there be perceived to be prisms, to which, in different points, were attached some small crystals of calcareous sulphate. To free, as much as possible, these crystals from the adhering mother liquor, they were washed with distilled water, and dried between leaves of blotting-paper. Although the remainder of the lixivium showed no disposition to deposit other regular crystals, I was not able to recognise any difference between the metal which it contained and that in the crystals: it was in both an oxide of nickel mixed with cobalt. The crystals, which weighed five half-ounces, were redissolved in 32 ounces of boiling water; the solution was evaporated to the formation of a thin pellicle, and, after having been filtered, it was put in the vicinity of a stove, to cool slowly and to crystallize. At the end of 48 hours the greater part of the salt was crystallized in beautiful tetraëdral and rhomboidal pyramids, short, and of a yellow green, of which the lateral surfaces formed an angle of 115 and 65 degrees, often with a truncated extremity, and always with an angle of 132 degrees of inclination towards the terminal surface. This result proves that the salt can more easily be formed in regular crystals by refrigeration than by slow evaporation. All these collected crystals having been washed with water were redissolved, and the separation of the oxide of nickel was effected by boiling it with carbonate of potash till the ammonia was disengaged.

4. As much for the sake of having this oxide free from carbonic acid, as for judging if it was purged of cobalt, I dissolved it in nitric acid, and treated it afterwards with pure ammonia, in the manner which has been often mentioned. I evaporated to dryness the beautiful blue liquor which had been freed by filtration from a residue of five grains, which showed itself to be an impure oxide of cobalt. After a subsequent solution there was deposited an oxide of a beautiful clear green, which, after being washed and dried, weighed a half-ounce. The liquor which passed through the filter was analysed by carbonate of potash at a heat of boiling water, which still gave 170 grains of oxide of nickel containing carbonic acid of a pale green. I dissolved a little of it in muriatic acid, and applied the solution to paper. On heating it afterwards, the stains became yellow, inclining only a little towards green. But when the oxide of nickel, which had been separated from the same during evapora-

 tion,

tion, was dissolved, disengaging much of the oxygenated muriatic acid, on being spread upon paper, and warmed, it showed the colour of a sympathetic ink well saturated with cobalt; from whence it follows that it was more rich in cobalt than that collected by precipitation.

The oxides, collected in a different manner, were dissolved in the nitric and sulphuric acids after becoming gray. I believed that this happened because the oxide of the nickel had been perhaps the first dissolved, and because the oxide of cobalt was, at least for the most part, the last remaining; which, however, was not confirmed by the experiments made on this subject. When exposed to a low red heat, these oxides changed their colour into a blackish gray, and then (as was also the case by the addition of sulphuric acid) they threw down some residuum by evaporation of the nitrous acid, which was also separated from it by the addition of an alkaline lixivium. In other respects it acted with ammonia, &c. in the manner that has already been mentioned.

The following is the result of what has been so far stated:

a. The sulphates and the nitrates of ammoniacal nickel drawn from the ore of cobalt contain always some cobalt in their composition: it is impossible by the process of Hermstadt, modified in the preceding manner, to have the oxide of nickel without a mixture of cobalt.

b. By partially decomposing the nitrate of ammoniacal cobalt by evaporation, there is obtained an oxide of nickel very rich in cobalt which contains nitric acid, and the oxide of nickel which is found in this salt not yet decomposed contains a very small quantity of cobalt.

B. Dr. Schnaubert has shown (*Journal de Pharmacie*, par Tromsdorff, vol. ii. no. 2. p. 66.) a process for obtaining an oxide of pure nickel; that is, to dissolve the metal of nickel mixed with cobalt, or its oxide already disengaged from other substances, in nitric acid, to precipitate it by carbonate of potash, and to expose it to a white heat after washing and drying. In this manner he always obtained a yellow oxide, on which he afterwards boiled sulphuric acid sufficiently strong, which gave him a solution of oxide of nickel of a grass green, while the oxide of cobalt showed itself in the residuum under a yellow colour. He proved the purity of the sulphate of nickel prepared in this way, by the property which ammonia had to precipitate it of a clear green colour, and, when added in excess, to redissolve it of a beautiful dark blue colour. This argument will

will appear insufficient to those who know that the oxide of nickel, mixed even with many hundred times its weight of cobalt, does not experience any sensible change of colour in its precipitations, nor in its solutions with ammonia; and besides, he has not pointed out the means by which he was convinced that the oxide obtained in the residuum of the solution in the sulphuric acid was an oxide of cobalt. The vague precept, to heat the oxide obtained, without giving the least information on the degree of heat; the uncertainty in which he leaves us respecting the sulphuric acid of which he made use; all these circumstances throw upon the exactness of the indicated process a doubt which the following experiments alone will be able to clear up.

1. A portion of oxide of carbonic nickel A, was exposed, during an hour, to a violent fire nearly of a white heat. The oxide, when yet warm, was brownish yellow. After cooling it took a gray colour inclining to yellow, but not entirely so. The oxide of nickel obtained by the evaporation A 4, having been treated in the same manner, was yet a little more gray than the preceding. The oxide of carbonic nickel was again exposed, for half an hour more, to a white heat: while hot it was yellow inclining to brownish, but when cold it was gray inclining to a brownish yellow.

2. Thirty grains of this torrified oxide were kept some hours in digestion with ninety grains of pure sulphuric acid of the specific gravity of 1·860. Having been afterwards heated, the mass swelled up with a noisy ebullition, and presented a yellow substance inclining to green. By ebullition with a half-ounce of water it was dissolved, leaving nearly a grain of powder of a gray yellow, which proved to be oxide of nickel mixed with cobalt, and a little impurity. I obtained exactly the same result, with the same appearances, in treating a second time in the same manner, and with 90 grains of concentrated sulphuric acid, 35 grains of oxide of nickel, which I had obtained by heating briskly, even to redness, 60 grains of nitrate of ammoniacal nickel prepared by evaporation. By heating the same oxide to whiteness, in a fire urged with bellows, for half an hour, I did not obtain a yellow mass, but one of yellowish gray, inclining a little to green, which acted with the sulphuric acid, as I have formerly stated.

3. I repeated afterwards the same experiment with sulphuric acid weakened. 160 grains of oxide of carbonic nickel were exposed, during a half-hour, to a very violent white heat; after which they still weighed 75 grains. This

substance was of a greenish yellow here and there, and of a blueish gray at the points of contact with the crucible. On being bruised it gave a powder of a black gray. It was mixed with a gros of sulphuric acid diluted with five gros of water. After a sufficient ebullition water was added, and the solution was decanted clear. The residue was treated with weak sulphuric acid. There was instantly a brisk disengagement of gas, and by afterwards heating this mixture it manifested evidently the odour of hydrogen gas. After a sufficient ebullition water was added, and the solution was decanted clear. The residue was treated anew with weak sulphuric acid. This gave then ten grains of a residue, which was not an oxide of cobalt, but an oxide of nickel mixed with cobalt, as the solution proved in the acids and ammonia.

The two preceding solutions were each a part analysed by pure potash, and the precipitate was besides heated with an excess of potash; afterwards washed and dried. On trial each of the precipitates discovered cobalt, which was always found purer in that of the first solution; for the solution in the muriatic acid, applied on paper and heated, inclined sensibly to yellow, while the precipitate of the second solution gave a writing of a clear and pure green. It is astonishing that the precipitate of the former solution has furnished more oxygenated muriatic acid than that of the second.

The reported experiments, and some others like them, of which I have not spoken, prove:

A. That oxide of nickel, weakly or violently warmed, does not take the yellow colour; and that if this colour has been observed by Mr. Schnaubert, it must depend either on a certain connection of elements which entered the composition of his oxide, or perhaps upon a mixture of a little arsenic.

B. That we cannot, by the aid of the process of Mr. Schnaubert, obtain an oxide of nickel exempt from cobalt, since it does not even occasion a separation of the two oxides sufficiently far to be sensible to the eye.

C. I pass over in silence several experiments which I have made for finding a sure and exact method to produce this separation, because they have not conducted me to the desired end, or presented me with any other interesting phænomenon: they tended chiefly to point out an acid which with one of the oxides formed an insoluble salt, and with the other a salt easily dissolved. It only remained for

for me, therefore, to return to the process already pointed out, A, which consists in a partial decomposition of nitrate of ammoniacal nickel: for that which has been proposed by Lehman, to melt the nickel mixed with cobalt from fifteen to twenty times, to a commencement of vitrification, for the purpose of scorifying all the cobalt; as well as that pointed out by Bergman, to repeat the melting three or four times with eight or twelve times as much of pure nitre, was too troublesome and expensive. In consequence, I repeatedly treated the oxide of nickel, which by means of carbonate of potash had been separated from the triple salt not dissolved by the former evaporation, in such a manner that, after having dissolved it by nitric acid, I had recourse to ammonia and to evaporation, as explained above.

It is thus that I finally obtained, entirely exempt from cobalt, the oxide separated by potash from the triple salt which had been redissolved after evaporation.

The oxide which was separated by the evaporation of the nitrate of ammoniacal nickel was in the former operation already entirely purged of cobalt; only it still contained, as has been observed, a little nitric acid. Oxide of nickel, which, after having been reduced to a state of nakedness by evaporation, contains still some cobalt, is naturally susceptible of undergoing anew the same operation.

By this method we may be served with the article in question, until our further discoveries have shown us one more expeditious. It does not occasion any considerable expense; for by means of potash we may effect, in a retort, the evaporation of nitrate of ammoniacal nickel, the same as the subsequent decomposition of the triple salt, and thus save the ammonia for other uses. As is the case in works on a great scale, we can also save a part of the nitrate from the former operation by the evaporation of the water used in washing it.

XL. *An experimental Inquiry into the Nature of Gravelly and Calculous Concretions in the Human Subject; and the Effects of Alkaline and Acid Substances on them, in and out of the Body.* *By* THOMAS EGAN, *M.D. M.R.I.A.**

THE constant occurrence of these afflicting complaints in Simpson's Gouty Hospital, to which I have been physician for several years, first turned my serious attention to the most

* From *Transactions of the Royal Irish Academy.*

most probable means of alleviating or removing them. But, to obtain this desirable end, an examination into the nature of the predisposing and proximate causes; of the chemical and other properties of gravelly matter itself; and that species of calculus most generally resulting from its aggregation; as well as of the remedies, and their mode of operation, became indispensably necessary. I must also acknowledge that I was not a little excited to this inquiry by the consideration, that, whilst the medicines now most confided in by modern practitioners are supposed to exert no energy on those substances out of the body, yet their beneficial effects, taken internally, stand uncontroverted by the experience of almost every physician.

Induced by these motives, I had, as far back as the year 1799, instituted a series of experiments in hopes of throwing some more light on this subject; and, perhaps, chemically explaining upon what ground alkaline substances in general alleviate, whilst acids as constantly aggravate, this afflicting disease.

But, knowing that Messrs. Fourcroy and Vauquelin had been, for many years, particularly engaged in the analysis of urine and its morbid concretions; and expecting, from their superior abilities in researches of this kind, that the object which I had in view would be more satisfactorily fulfilled, I did not wish to intrude any observations of my own on the public.

After, however, most anxiously attending to the result of their scientific labours on this subject, as they have been, since that period, successively detailed, by M. Fourcroy, in the *Annales de Chimie*, Memoirs of the National Institute, and latterly in his great and elaborate work the *Connoissances Chimiques*; and finding little, if indeed any things illustrative of the subject, to which I would wish to point the attention of the faculty as well as the public in general, I again latterly repeated, with much care, my experiment, of 1799, and added some more, which may probably prove interesting in a practical point of view.

These, with some observations, and deductions from them, I now, with diffidence, offer to the candour and consideration of the academy.

I must here premise, that the limits of an academic dissertation necessarily confine me chiefly to the consideration of gravelly matter itself, and that species of calculus which most generally results from its aggregation.

Though determined to intrude as little as possible on their time by an useless quotation from antient authors, who

who could have no clear ideas of the subject; yet the better illustration of my object, as well as a sense of justice, oblige me to go as far back as Van Helmont, whose great though eccentric genius first observed that the subject matter of calculus existed in the urine itself. But the flighty extravagance of his ideas, of which he has given us a specimen on this subject in his *Treatise de Lithiasi*, (a wonderful production for the time,) caused little attention to be paid to his opinion; and it was reserved for the capacious and learned genius of Boerhaave first to ascertain, beyond future doubt, the presence of gravelly matter as a natural constituent part of urine, kept in chemical solution in it, and eliminated by it out of the system. Of this important fact no material use was made, until the all-prying genius of the immortal Linnæus induced him to request his friend Scheele to turn, for a moment, his great chemical abilities to the investigation of this subject: with what success is but too well known. And from this again had arisen the further prosecution of this inquiry by the celebrated Bergman.

The result of the analysis of the latter was highly honourable to the former chemist, as they perfectly agreed in almost every particular, with the exception of some small quantity of insoluble matter, and the presence of lime, observed by Bergman: a difference now very easily accounted for; the former having examined calculi of the pure lithic acid, or, as it is now termed, uric kind, (by far the most common species,) and entirely soluble in pure alkaline lixivia and nitric acid; the latter, those of the mixed kind, consisting also chiefly of lithic acid, but with interposed laminæ; or probably a nucleus of either calcareous phosphate or oxalate of lime, which frequently occurs in a very large proportion of these concretions. We may also observe, that Bergman had not, at this period, an adequate idea of the large proportion and insolubility of animal matter contained in them.

From their joint analysis it was, for the first time, proved that the subject matter of gravel, and of a very large proportion of calculi, was present in a state of real chemical solution in all healthy urine; that it was possessed of the following distinguishing chemical properties:

Insipid, inodorous, crystallizable, nearly insoluble in cold water, and only soluble in some thousand times its weight of boiling water; separable again from this, upon cooling, in a beautiful and peculiar crystalline form; of easy solubility in pure alkaline lixivia, which it renders sweetish, and neutralizes; precipitable from these again by the weakest

weakest acids, and still possessing its original crystalline form and properties. That, from these circumstances, with that of turning the vegetable blues red, it was of an acid nature, soluble in nitrous acid with effervescence: this solution tingeing the skin and other animal matters red, and, upon evaporation to dryness, assuming a red rose colour: this last property being peculiarly characteristic of this substance; subliming in part by distillation, without any alteration in its properties, and affording carbonate of ammonia, and other usual animal products, partly from the admixture of animal matter, and probably some adhering urea. To these distinguishing chemical properties of the Swedish chemist, Fourcroy has since added the following: When triturated with a lixivium of either of the fixed alkalies, it forms a matter of a saponaceous consistence, very soluble with excess of alkali, but little so without it. The saturated urates of potash and soda are little sapid, soluble, or crystallizable. By precipitating their dilute solution by muriatic acid we obtain the lithic acid in brilliant needle-like crystals, very voluminous, a little coloured, tending to the yellow, or *fauve*, as he calls it. Ammonia exerts little, if any, solvent power upon it: lime water takes up a little. The alkaline carbonates have no action upon it: and this last circumstance, I would beg leave to observe, has continued to be the opinion to this day; but how far founded, will appear in the sequel. To this matter Scheele gave the name of *lithic acid*; by which it continued to be known, until our countryman, Dr. Pearson, has latterly proposed that of *uric*; a change greedily adopted by the French chemists, as being more particularly indicative of its origin. In compliance with the philosophers of both nations, I shall, in future, term it *uric acid*, and the concretions of that nature, calculi of the uric acid kind. The publication of Scheele's Essay excited the experimental inquiries of both chemists and physicians. His experiments were, accordingly, repeated by several of our countrymen in particular; but with various, and in many instances different, results.

It was already cursorily observed, that Bergman's analysis differed from Scheele's in some circumstances, which he, even at that period, was disposed to attribute to a difference in the nature of the calculi which they respectively examined; and this conjecture has been fully established by every subsequent inquiry since that time. We accordingly find a paper of Dr. Dawson's, in the London Medical Transactions for the year 1769, showing these concretions to be of very different and opposite kinds, and, of course, soluble

soluble in very different and opposite kinds of menstrua: as also a letter from Dr. Saunders to Dr. Percival, of Manchester, published in the third volume of Percival's Philosophical and Experimental Essays, in 1776, detailing several experiments; from which he fairly concludes that the doctor's enthusiastic hope, of dissolving all calculi in a solution of carbonic acid, must prove groundless, from the very different nature of their component parts, as ascertained by his own experiments. This was placed beyond further doubt by our own learned and ingenious professor Mr. William Higgins, who, in an analysis of a calculus, of which he gives an account in his Comparative View of the Phlogistic and Antiphlogistic Theories, (a work of singular merit for that period, to which we will afterwards refer,) and published so far back as 1789, enumerates the many various substances contained in one specimen only. The researches of Austin, Lane, and Brugnatelli, led to similar results. But to the learned and accurate Dr. Wollaston we stand indebted for the first clear and distinct discrimination of the component parts of these substances. In a paper read to the Royal Society in the year 1797, which would not discredit either a Bergman or a Klaproth, he has most accurately demonstrated, both analytically and synthetically, the component parts of three distinct species of calculi; namely, the fusible, as he terms it, or the ammoniaco-magnesian phosphate of Fourcroy; the mulberry, or oxalate of lime kind; and bone earth calculus, or phosphate of lime, which, with the uric, well known to us since the time of Scheele, left us then acquainted with the four species of calculi of most frequent occurrence. Under these circumstances I cannot help expressing my surprise at finding M. Fourcroy still assuming the merit of the discovery of all the different component parts of calculi, the uric acid and phosphate of lime excepted. This circumstance must appear the more unaccountable, when we consider that the communication of Dr. Wollaston's experiments was through the medium of the Transactions of the Royal Society for 1797. Finally, M. Fourcroy, to whom Europe stands not a little indebted for the present general diffusion of chemical knowledge, and to whom the medical profession owe the greatest obligations for his unremitted application to animal chemistry, has, in conjunction with Vauquelin, given us the result of his researches upon five hundred calculi; from which it appears that they contain the seven following ingredients:

1. Uric acid.
2. Urate of ammonia.

3. Phosphate

3. Phosphate of lime.
4. Ammoniaco-magnesian phosphate.
5. Oxalate of lime.
6. Silica.
7. Animal matter.

From the prevalence of any of these ingredients, or their relative proportions, he divides them into four genera; and these again into twelve species: for an account of which I must refer to the tenth volume of the *Connoissances Chimiques*, and the Memoirs of the National Institute; not proposing to go into their chemical properties further than may be necessary to my present inquiry; namely, of how far acids may be conducive to the formation, or alkalescent substances to the prevention, or even solution, of a large proportion of gravelly and calculous concretions. We have already remarked, that to the sagacity of Boerhaave we are indebted for the knowledge of gravelly matter being a constituent part of urine kept in chemical solution in it; and, happily for mankind, only separable from it after being some considerable time out of the body. After minutely detailing the ingenious means made use of by Boerhaave to ascertain this important point, to which I beg leave to refer, his commentator, Van Swieten, goes on to observe:

"Hoc calculi rudimenta adsunt etiam in urina hominum sanissimorum; quæ, si una cum urina secernuntur, antequam ab urina secesserint, et concrescere inceperint, nullo modo sanitatem lædent. Cum autem observatum fuerit, illam separationem rudimentorum calculi citius fieri in quibusdam hominibus, tardius in aliis, patet, illos magis calculo obnoxios vivere, in quibus citius hæc separatio arenularum obtinet. An quandoque illa separatio contingit jam in renibus, et in vesica, antequam urina expellatur de corpore? Certe videtur. Vidi sæpius, una cum urina excretum sabulum nephriticum expulsum fuisse, statimque, calente adhuc et fumante urina, in fundo matulæ subsedisse. Contigit aliquoties, inventam fuisse, in linteis sanorum infantum urina madidis, copiam sabuli nephritici, satis duri, quod videtur una cum urina excretum fuisse. Cum enim magna cura haberetur, ne hi infantes, (illustri genere nati,) diutius urinâ, vel aliis sordibus, conspurcati manerent, et urina statim per lintea penetret, vix videtur possibile fuisse, ut in urina jam emissa hoc sabulum productum fuerit, intra unam alteramve horum."

And again he adds: "Hoc sabulum, in urina etiam sanissima concrescens, vocari posset calculus nativus; a quo nemo liber est; at qui tunc tantum metuendus videtur, si cito

cito in urina concrescat. Felices illi, in quibus tardissime hoc fit. Propriam sæpius examinavi urinam, lætusque vidi, rudimenta illa prima calculi separari quam tardissime, requiri quandoque horas viginti quatuor et ultra, antequam in sabulum majoris molis concrescere potuerint. Sed et, licet decimum tertium ætatis lustrum emensus jam fuerim, ab omni lithiasi immunis vixi."

The mode and appearances attending the separation and crystallization of this substance from healthy urine, is one of the most beautiful that, probably, chemisiry affords. But, as the circumstances are so minutely and correctly detailed by Boerhaave, and his commentator, Van Swieten, in his treatise *De Calculo*, vol. v. p. 201 and 202, and correspond so much with my own experiments, so often repeated, I must refer to him. On this passage, however, I must observe, that the space of twenty-four hours, mentioned by him as the period of spontaneous separation, is by far, in the healthy state, too short, and that it extends to two, three, and sometimes more days, according to the existing temperature and other circumstances. Nothing, therefore, I will presume to say, is more erroneous than the assertion, repeated in almost every chemical book, that the uric acid separates from urine upon cooling. When this occurs, which frequently happens, particularly with children, the urine is certainly surcharged with this very insoluble substance.

An increased temperature hastens the incipient decomposition of urine, and its first ammoniacal degeneration is always attended by the deposition of its uric acid in its crystalline form.

This did not escape the observation of Hales, who tells us, that urine, tending to putrefaction, affords most of this acid substance; and, indeed, were it to be deposited upon cooling, or within the space of twenty-four hours, or even more, as is so generally asserted, it should every day present itself to physicians, who so constantly attend to the state of urine in glasses; but this is by no means the case: and we find Fourcroy, in his last publication, mentioning from twenty-four to forty-eight hours, which certainly only applies to summer heat, or the circumstance already mentioned.

Our next great obligation is, undoubtedly, to Scheele, who has made us acquainted with its nature, and the very distinct chemical properties already enumerated.

While in the state of gravel it is ever the same, whether passed

passed immediately with the urine, or spontaneously deposited, or precipitated from it; a circumstance that, for a long time, continued to give me much surprise, considering the variety of calculi; but of the truth of which I was convinced by the examination of many hundred specimens, for many years back.

I was therefore pleased to find, that Fourcroy, for the first time, in his *Connoissances Chimiques*, asserts, "les sables des reins sont presque toujours de l'acide urique." And in another place he says, speaking of the uric acid, "c'est lui qui forme les sables, qui se crystallize, et s'attache aux parrois de vaisseaux."

No wonder, then, that calculi of this kind should be of most frequent occurrence; and that, of five hundred analysed by Fourcroy, one-fourth should entirely consist of it, besides its occasional admixture with the remainder; and of three hundred, examined by Pearson, the greater number were found to be of this nature.

Having premised these necessary observations, we have now to consider to what circumstances we may attribute its separation, in a crystallized or aggregate state, from its natural solvent; the only condition in which it can be productive of inconvenience, or diseases of this kind. And first, I would observe that, being a natural secretion, of which the urine is only the vehicle destined to carry it out of the system, it must be subject to the same derangements with the other secretions of the human body, and may, of course, sometimes exceed in quantity, and at other times be more deficient; which last circumstance seems to take place during the continuance of acute diseases.

That a morbidly increased secretion does frequently occur, and that, too, independent of external causes, we have the most satisfactory proof of in the hereditary dispositions of many families to this complaint: and, indeed, when we consider the same to take place, relative to the functions and secretions of the liver, we must not be surprised at similar deviations in those of the kidneys. Here, truly, they are of more mischievous tendency, as, from the very sparing solubility of the uric acid, (even in its own natural menstruum,) the smallest excess in quantity must subject it to precipitation.

Having premised these necessary considerations, I shall proceed to inquire into those circumstances which the experience and observation of all times have pointed out to us as the most frequent occasional causes of these maladies,

 and

and how far these opinions may be confirmed by experiments instituted for that purpose.

And first, It is a matter of notoriety, that the period of life, from infancy to about fifteen inclusive, is most subject to disorders of this kind.

Of this practical observation we have an interesting confirmation inserted in the second volume of the Memoirs of the French National Institute, Mathematical and Physical Sciences, year 7. Under the former happy regime there was instituted, about forty years ago, at Luneville, in Lorraine, an hospital for the exclusive relief of calculous and gravelly patients. In that interval, 1629, of both sexes, were admitted, and operated upon. Of these, 1564 were males, and only 65 females.

C. Saucerotte, an associate of the Institute, to whom we are indebted for these interesting details, annexes tables indicative of the number of these patients, that occurred at the different periods of life, from the age of one up to seventy-eight. To these, as too extensive to be inserted here, I would beg leave to refer; and shall satisfy myself with some extracts only, expressive of the general result.

Age of Patients. Male Sex.	Number of Patients.
1 year to 2	1
2 years	14
3	79
4	131
5	145
6	147

From this age, which afforded the maximum of the number of patients, we find a gradual declension, as follows:

Age of Patients.	Number of Patients.
8 years	121
10	79
15	39
20	16
25	7
30	8
35	4
50	5
60	2
70	2
78	1

Of

Of the sixty-five females,

Age of Patients.	Number of Patients.
1 year to 3	1
4	8
5	7
6	4
7	6
8	5
9	3
12	4
14	1

From which period, down to seventy-eight, there occurs but one or two upon each year. From these, then, we learn how much more subject the male sex is to those complaints than the female; and the earlier periods of life, than the more advanced. For among the males in the sixth year we find 147 (the greatest number), and among the females only five at eight. From these periods, in both sexes, the numbers rapidly diminish.

These facts would lead us to conclude that some physiological cause, peculiar to the functions of this early stage, may give rise to this difference; and I will not pretend to say but this may possibly exist: but when we consider that in every country the infant poor are the greatest sufferers, we are induced to inquire further, and suspect the existence of some general cause affecting and applicable to them all. That a similarity of diet (in the children of this class of society, in particular) must every where nearly take place, is evident; and that this is, but too often, of the kind most prone to the acescent tendency, such as pap, gruel, sour milk, &c.; all which it is not always in the power of the parents to renew, or administer, in a recent and sound state; an error not unfrequently occurring from the negligence of nurses even in the upper ranks, but irremediable in the lower; where this acescent tendency cannot be corrected by the seasonable admixture of broth, or other light animal food; their unhappy situation confining them exclusively, like their cattle, to the sole use of vegetables and the farinacea.

To pass on from infancy to the advanced periods of life, and begin with our own island, we find that, considering the extent of our population, the disease is of relative rare occurrence: so much so, that the late Mr. Dease, (whose premature death we have still to deplore, as a national calamity,) with all his well deserved celebrity as a lithotomist, never operated upon more than sixty. A small number, indeed,

indeed, when we consider that the operation is seldom, if ever, attempted in the country. And why this should happen here, we shall be presently, perhaps, better able to judge.

The reverse of this occurs in the sister kingdom; and the Irish student feels astonished at the frequency of the operation in all the London hospitals, though also performed in those of the more considerable country towns; and, upon inquiry, he finds that a large proportion of these patients come up from the cider counties of Hereford, Devon, &c.: and it must naturally occur to him, that the general use of fermented liquors of every kind, beer, cider, perry, and factitious wines, which prevail in England, renders the disease of more frequent occurrence there than with us, the great mass of our people being deprived of these luxuries.

If we pass over to the Continent, we find our neighbouring provinces, Picardy, Normandy, and Britany, in particular, still more subject to affections of this kind; so much so, that the late Mr. Dease could not give credit to the extraordinary number of patients operated on, in one year only, in the hospital of Rouen; though many must have, of course, repaired to Paris. The same, though in a lesser degree, takes place in Champagne; and it is almost unnecessary to observe, that the general beverage of the northern provinces consists of cider, or of poor wine, equally acescent in its nature, and prone to the acetous fermentation. The Champagne, though somewhat less so, is replete with carbonic acid gas and disengaged tartarous acid; and though, in the more southern provinces, this malady cannot be considered as endemial, yet it is of frequent occurrence in the hospitals of Montpellier.

For, even in these favoured climes, where wine is of so little value, and withal so spirituous, the unfortunate peasant is obliged to content himself with an inferior quality, prepared by a second maceration of the *marc* of the grape, which he denominates *picquet*; a *patois* appellation, most happily applied to its highly acid quality.

In that once happy country, Switzerland, on the contrary, as baron Haller assures us, the disease is by no means frequent, and chiefly confined to the children of the poorer sort; their mountainous and elevated situations affording them little or no vinous liquors; whereas their neighbours, the inhabitants of the Rhine and Moselle, as well as some tracts on the banks of the Danube, are peculiarly afflicted.

The truth of this observation we find confirmed by the

 medical

medical authors of all times. Silvius observes, "Vina acida tenuia et Rhenana, magis nocere calculosis quam opima;" and the same is particularly insisted on in Dolæus's "Encyclopædia Ephemerides Naturæ Curiosorum," and Rivinus's "Morbi Endemici," &c. Now, the wines in these countries are well known to be of an acid quality: and Hoffman asserts, and that too from experiments, that they abound in the tartarous acid, having found them to contain a double relative quantity of that in other wines; and to this we may add no small proportion of carbonic acid. Linnæus, in his dissertation "De Genesi Calculi," inserted in the second volume of the "Amœnitates Academicæ," seems more particularly to point out acids, and acescent drinks, as the chief causes of calculous affections. He says: "Acida fermentescentia omnia calculum promovent; hinc vina acida genesi calculi magis favent, quam dulcia. Qui acida vina copiose ingurgitant, podagræ et calculo plus exponuntur, quam illi, qui terras calidiores inhabitant, et dulcia vina hauriunt. Nec mirum, cum vini Rhenani libræ quatuor destillatione dant spiritus acidi drachmas quinque; et vini Tocariensis præbet spiritus acidi tantum semidrachmam, teste Hoffmanno. Sanissimus quisque a potu acido sæpe stranguriam incurrit, eo quod ab acidis ingestis particulæ terrestres præcipitantur." And again: "Quin podagra igitur et calculus ab acido generentur, nullum est dubium, id etiam ab eorum communi cura, ad quam pergimus, luculentius patebit." Beverovie, *De Calculo*, 80, also observes: "In nullo vino tantum tartari apud nos accrescit, quam Rhenano. De me ipso, quod etiam ex plurimis audivisse memini, possum testari, nunquam Rhenanum assumsisse paulo largius, quin copiose arenulas excernerem."

The reverse of all this is observed to take place where the use of wine is prohibited. Rivinus observes, that in the city of Batavia, where the pursuit of commerce brings together a vast assemblage of the neighbouring Asiatic nations, whenever the disease occurs, it is almost always in the instance of some Hollander, who, in his passage to India, drank freely of bottled beer, and used sour crout. In Persia, the same author, in his excellent treatise *De Morbis Endemicis*, observes, that whenever calculous affection occurs, either in Ispahan or the provinces, it is assuredly in the instance of some Armenian; fellows, (to use his words,) who, in every latitude, drink more wine than water.

Again, in Grand Cairo, where the proximity of the Grecian islands, and ready conveyance by the Nile, render wine

wine of easy acquisition, and drunkenness and public houses as common as in any city in Germany; we learn, from Prosper Alpinus, that the disease is of very frequent occurrence; for, besides a mixed population of Franks, Armenians, Arabs, &c., the Mamelukes, as well as many other Turks of the higher ranks, do not, in deference to the Mahometan law, refrain from wine. The Cyprian and Grecian wines, if not adulterated, or become acescent by dilution, and the warm temperature of that city, are, in themselves, among the least objectionable. But, when we consider that Paris is chiefly supplied with Burgundy, and that yet in no part of the world does there occur more mischief from the attempts to keep down and correct its acescency, we shall easily form an opinion of the quality of the wine retailed in Cairo.

To this abstinence, then, from wine and fermented liquors; as also, perhaps, to the admixture of a large proportion of the warmest spices in their vegetable food, tending to correct its acescent tendency; we may ascribe the rare occurrence of this disease in the more southern climates.

Now, these more general remarks we find peculiarly to coincide with the observations of the patients themselves, as well as that of the physician; for such as have laboured under these complaints a sufficient length of time to become acquainted with the *juvantia* and *lædentia*, most scrupulously abstain from acids, and acescent drinks of all kinds, and, what they find most particularly pernicious, beer or ales turning over to the acetous fermentation, or *hard*, as they are generally termed. And, indeed, nothing is more common, than that an indulgence in cider, claret, or acidulated punch, nay, a draft of hard beer or porter, should be followed by a fit of the gout and gravel.

The connection between these diseases forms an interesting and curious subject of physiological as well as pathological inquiry; but, proposing to offer some observations on this subject on a future occasion, I shall at present decline entering upon it, and pass on to observe, that the bad effects of all acidulous drinks are fully confirmed by the experience of our many sufferers in Simpson's hospital. Hewson, who lately died there at the advanced age of 102, never tasted the beer of the house during the summer months, and substituted milk for it; being taught by experience, that its acid tendency, during that period, always induced his gravelly paroxysms. And Clapham, who suffered much from gout and gravel, and was for many years

 a ship

a ship captain, informed me his voyages to America were always succeeded by fits of both; which he attributed to a free indulgence in the use of cider, a beverage to which he was then peculiarly attached: and that, at any time, he could excite a paroxysm of one or the other, or both, by drinking acidulated punch, or claret. Khensk our greatest martyr (having all his articulations distorted by gouty concretions, and who once lived in easy circumstances,) assured me that the severest and longest protracted fit of the gout and gravel he ever experienced was occasioned by a surfeit of a poor vapid claret. And I shall conclude this part of my subject by observing, that the clergy of the Roman catholic church are peculiarly liable to these complaints, and form no small proportion of the number operated upon in this city; which I would attribute to the use of a small and sour wine during their residence in their seminaries abroad.

[To be continued.]

XLI. *Twenty-fifth Communication from Dr.* Thornton, *relative to Pneumatic Medicine.*

To Mr. Tilloch.

Hinde-street, Manchester-square,
November 1, 1805.

DEAR SIR,

The number of cures performed by the pneumatic practice, after the usual routine of medical agency has failed, are now become so very numerous, that I am certain the record of the whole would fill a large volume, so eager have been the afflicted to seek resource from the *aërial remedies* in diseases probably otherwise incurable.

In the family of Mr. Wilson, sadler, the subject of the last communication, two cases have occurred which yet further evince the efficacy of the *pneumatic practice.*

A Decline cured by Vital Air.

Elizabeth Barlow had been in a declining state of health for four years, frequently sick, weak, and debilitated; and a medical gentleman, a relation, pronounced to the parents the improbability of her long surviving. The vital air, two quarts, diluted with atmospheric, was now tried with tonic medicines: it was observed from the very first day that her appetite and looks were improved. In six weeks she was restored to health, and has since continued well. It is now two years.

Another cured by Vital Air.

Miss Ann Russel, also, niece to Mr. Wilson, a beautiful young lady, æt. 20, fell into a decline, or wasting, which continued for the space of nine months. She was reduced so low as hardly to be able to walk across the room; her bones nearly came through her skin; a total loss of appetite, with dejection of spirits, took place, but no cough. As medicines seemed to have no effect in arresting the progress of this decay, the vital air was had recourse to, which, to the astonishment of all, produced a very speedy cure. The vital air, a gallon per diem, each time of inhaling it diluted with four parts of atmospheric, produced a glow over the whole frame; the appetite returned, the spirits of course were increased, and the young lady gained flesh. It is now eight months, and she has all along continued in perfect health.

Observations on these Cases by Dr. Thornton.

The functions of life often lag, as it were, without disorganization of parts, and this from an impoverished or disoxygenated state of the blood. Tonics only half do their office, unless the vital principle in the blood be increased; and though only inhaled *once a day*, yet if we calculate that the momentum is always going on, and that only one impulse to a wheel in motion accelerates all the movements for a given time, therefore this one impulse in the animated machine has a great effect: thus the stimulus of one or more glasses of wine *once a day* has a diurnal effect, if I may so use the expression, on the animated machine.

I have the honour to remain, dear sir,
Your faithful obedient servant,
ROBERT JOHN THORNTON.

XLII. *On a new Metal recently discovered by M.* TROMSDORFF[*].

M. TROMSDORFF received the fossil containing the new metal of M. Counsellor Thon, of Eisenach, who found it in a mass of rock. The fossil was of an iron gray colour, very weighty, and had a scaly fracture, exhibiting, when seen through a magnifying glass, holes and stripes of a deep yellow.

[*] From *Journal de Chimie et de Physique*, tom. v.

M. Thon took this fossil at the first view for anthracite: he held a small bit of it above the flame of a lamp for the sake of assuring himself of its incombustibility; but it inflamed, took fire, and burnt with a pale blue flame. Some drops flowed, which were crystallized in stars, and were surrounded with a yellow rim. The mineral, when melted further, kept the same colour. On dropping some nitric acid on some bits of the mineral, the acid did not appear for some time to attack it; but by the aid of heat it changed it to a reddish brown, and by ebullition dissolved it, taking to itself a yellow colour. To assure himself that the mineral did not contain any mercury, M. Thon rubbed it on a plate of silver, to which it communicated a yellow colour.

M. Tromsdorff tried, with the small quantity of the mineral which he had been able to procure, the following experiments:

1. Five grains of the mineral were put into a small cylinder of glass, open on one side and exposed to the action of fire. They melted like wax, and spread a very strong sulphurous odour.

There was deposited on the sides, at the top a white sublimate, lower down a sublimate of yellow sulphur, lower still an orange-coloured sublimate, and finally a crystalline black sublimate, having a metallic lustre. The whole was then volatilized—the cylinder was put sideways.

2. Five grains of the fossil were exposed to heat with some nitric acid: the mineral acquired a red-brick colour, and the acid attacked it strongly, disengaging nitrous acid. The solution was evaporated to dryness, and a powder was obtained of a red colour which emitted a strong smell of sulphur. The powder was boiled with water, in which it was dissolved excepting nearly a grain, which had the colour of sulphur, and which burnt with the usual flame and smell of that combustible.

3. The solution (2.) contained some free nitric acid, and perhaps also sulphuric acid. The redundant acid was saturated with ammonia, and the solution was decomposed by several reagents.

a. Prussiate of potash produced a beautiful green precipitate.

b. Hydro-sulphuret of ammonia gave a shamoy-yellow precipitate.

c. Tincture of gall nuts gave a deep-brown precipitate inclining to blue, which after some time became blueish gray.

d. Caustic

d. Caustic ammonia in excess produced neither precipitate nor change of colour.

These experiments prove that there was no iron, no copper, nor nickel, in the mineral; but its manner of acting with the reagents, to which it was submitted, made M. Tromsdorff suspect the presence of a new metal, which the above experiment demonstrated, and that this metal was combined with sulphur, and was volatile.

4. The shining black sublimate was as well as possible separated from the yellow, and put to digest with nitric acid. It dissolved entirely, disengaging nitrous gas, and formed a clear and colourless solution. M. Tromsdorff then saturated the excess of the acid by liquid ammonia, and successively decomposed it in portions with the above reagents. He obtained the same results as before.

5. Some grains of the fossil were then heated with some nitro-muriatic acid. It dissolved entirely, except a light residue of a reddish yellow colour, which was apparently sulphur in combination with some metallic particles. The solution when filtered was at first clear, but grew turbid on cooling. It was saturated with ammonia, and decomposed by the same reagents as formerly. The results were the same.

6. Some grains of the fossil were put to digest with muriatic acid. It disengaged from it sulphurized hydrogen gas, and the fossil became brownish: however, the acid attacked it but slowly. The remainder of the fossil was put to boil even to dryness with colourless sulphuric acid concentrated, and the residue was diluted with water. It dissolved itself entirely, except a powder almost yellow, which, when collected on the filter, washed and dried, burnt like sulphur.

The solution had a reddish colour, and contained much free acid. It was tried by several reagents.

a. Prussiate of potash produced a green precipitate.

b. Hydro-sulphuret of ammonia produced a shamoy precipitate.

c. Tincture of galls, a blueish gray precipitate.

d. Caustic alkali, a white precipitate.

e. Carbonate of potash, the same precipitate.

7. The two last precipitates remained white when exposed to the air. They dissolved easily in acetic acid, with which they gave a colourless solution, which was divided, and submitted to the following experiments:

a. In a part of the solution was put a small polished piece

of copper money. After twenty-four hours the piece was covered with a grayish yellow crust; which, when rubbed with a burnisher, took the colour of shining iron.

b. He divided the remaining part into three portions, which he decomposed by prussiate of potash, hydro-sulphuret of ammonia, and tincture of galls: exactly the same results were obtained as in the preceding trials with the same reagents.

Having exhausted by these experiments his supply of mineral, M. Tromsdorff was obliged to state his researches. He concluded from the results, that the fossil forms a combination of sulphur with a new metal. But he says he cannot yet pronounce this with rigorous certainty. It is proved that the fossil contains a metal, as much by its manner of acting with the reagents, as by its precipitation by copper, and by the metallic aspect of the substance which it forms by sublimation. And it is not doubtful that it contains sulphur, since we can really separate this substance.

The first experiment proved that this metal is volatile, for it leaves not a single residue on the fire. We know no other metals that are volatile but mercury, bismuth, arsenic, zinc, and antimony. The metal in question cannot be mercury; for, besides its fluid form, this metal produces vermilion in combination with sulphur, of which it has not been possible here to discover the slightest trace. It cannot be bismuth, which is less volatile, and of which the acid solutions are precipitated by the prussiate of potash of a yellowish colour, by hydro-sulphuret of ammonia of a blackish-brown colour, and by galls of a yellow greenish colour. It cannot be arsenic, or that would have discovered itself by the smell, and which besides would not have yielded the phænomena observed. Still less can it be zinc, which requires a very strong heat to volatilize it, which is precipitated by the reagents employed by our author under quite different colours, and which, instead of being precipitated by copper, does itself precipitate that metal. Finally, antimony is precipitated of a white colour by prussiate of potash, and orange by hydro-sulphuret of ammonia, and its solution in sulphuric acid is not at all reddish, as is that of our metal. M. Tromsdorff concludes from all this, that the fossil he has examined is in all probability a combination of sulphur with a metal hitherto unknown.

XLIII. *On*

XLIII. *On muscular Motion.* *By* ANTHONY CARLISLE, *Esq. F.R.S.: being the Croonian Lecture. Read before the Royal Society November* 8, 1804.

[Concluded from p. 123.]

THE following animals were put into separate glass vessels, each filled with a pound weight of distilled water, previously boiled to expel the air, and the vessels inverted into quicksilver; viz. one gold fish, one frog, two leeches, and one fresh water muscle *. These animals were confined for several days, and exposed in the sun in the day-time, during the month of January, the temperature being from 43° to 48°; but no air bubbles were produced in the vessels, nor any sensible diminution of the water. The frog died on the third day, the fish on the fifth, the leeches on the eighth, and the fresh water muscle on the thirteenth. This unsuccessful experiment was made with the hope of ascertaining the changes produced in water by the respiration of aquatic animals, but the water had not undergone any chemical alteration.

Animals of the class mammalia, which hybernate, and become torpid in the winter, have at all times a power of subsisting under a confined respiration, which would destroy other animals not having this peculiar habit. In all the hybernating mammalia there is a peculiar structure of the heart, and its principal veins: the superior cava divides into two trunks; the left, passing over the left auricle of the heart, opens into the inferior part of the right auricle, near to the entrance of the vena cava inferior. The veins usually called azygos accumulate into two trunks, which open each into the branch of the vena cava superior, on its own side of the thorax. The intercostal arteries and veins in these animals are unusually large.

This tribe of quadrupeds have the habit of rolling up their bodies into the form of a ball during ordinary sleep, and they invariably assume the same attitude when in the torpid state: the limbs are all folded into the hollow made by the bending of the body; the clavicles, or first ribs, and the sternum, are pressed against the fore part of the neck, so as to interrupt the flow of blood which supplies the head, and to compress the trachea: the abdominal viscera and the hinder limbs are pushed against the diaphragm, so as to interrupt its motions, and to impede the flow of blood through the large vessels which penetrate it, and the lon-

* *Mytilus anatinus.*

gitudinal

gitudinal extension of the cavity of the thorax is entirely obstructed. Thus a confined circulation of the blood is carried on through the heart, probably adapted to the last weak actions of life, and to its gradual recommencement.

This diminished respiration is the first step into the state of torpidity; a deep sleep accompanies it; respiration then ceases altogether; the animal temperature is totally destroyed; coldness and insensibility take place; and finally, the heart concludes its motions, and the muscles cease to be irritable. It is worthy of remark, that a confined air, and a confined respiration, ever precede these phænomena: the animal retires from the open atmosphere, his mouth and nostrils are brought into contact with his chest, and enveloped in fur; the limbs become rigid, but the blood never coagulates during the dormant state. On being roused, the animal yawns, the respirations are fluttering, the heart acts slowly and irregularly, he begins to stretch out his limbs, and proceeds in quest of food. During this dormancy, the animal may be frozen without the destruction of the muscular irritability; and this always happens to the garden snail*, and to the chrysalides of many insects during the winter of this climate.

The loss of motion and sensation from the influence of low temperature accompany each other, and the capillaries of the vascular system appear to become contracted by the loss of animal heat, as in the examples of numbness from cold. Whether the cessation of muscular action be owing to the impeded influence of the nerves, or to the lowered temperature of the muscles themselves, is doubtful; but the known influence of cold upon the sensorial system, rather favours the supposition that a certain temperature is necessary for the transmission of nervous influence as well as sensation.

The hybernating animals require a longer time in drowning than others. A full grown hedge-hog was submersed in water at 48°, and firmly retained there: air bubbles began instantly to ascend, and continued during four minutes: the animal was not yet anxious for its liberty. After seven minutes it began to look about, attempting to escape; at ten minutes it rolled itself up, only protruding the snout, which was hastily retracted on being touched with the finger, and even the approach of the finger caused it to retract. After fifteen minutes complete submersion, the animal still remained rolled up, and withdrew its nose on being

* *Helix nemoralis.*

touched.

touched. After remaining thirty minutes under water, the animal was laid upon flannel, in an atmosphere of 62°, with its head inclined downwards; it soon began to relax the sphincter muscle which contracts the skin, slow respirations commenced, and it recovered entirely, without artificial aid, after two hours. Another hedge-hog, submersed in water at 94°, remained quiet until after five minutes; about the eighth minute it stretched itself out, and expired at the tenth. It remained relaxed and extended after the cessation of the vital functions; and its muscles were relaxed, contrary to those of the animal drowned in the colder water.

The irritability of the heart is inseparably connected with respiration. Whenever the inhaled gas differs in its properties from the common atmosphere, the muscular and sensible parts of the system exhibit the change; the actions of the heart are altered or suspended, and the whole muscular and sensorial systems partake of the disorder: the temperature of animals, as before intimated, seems altogether dependent on the respiratory functions, although it still remains uncertain in what manner this is effected.

The blood appears to be the medium of conveying heat to the different parts of the body; and the changes of animal temperature in the same individual at various times, or in its several parts, are always connected with the degree of rapidity of the circulation. It is no very wide stretch of physiological deduction to infer, that this increased temperature is produced by the more frequent exposure of the mass of blood to the respiratory influence, and the short time allowed in each circuit for the loss of the acquired heat.

The blood of an animal is usually coagulated immediately after death, and the muscles are contracted; but in some peculiar modes of death, neither the one nor the other of these effects is produced: with such exceptions, the two phænomena are concomitant.

A preternatural increase of animal heat delays the coagulation of the blood, and the last contractions of the muscles: these contractions gradually disappear before any changes from putrefaction are manifested; but the cup in the coagulum of blood does not relax in the same manner; hence it may be inferred that the final contraction of muscles is not the coagulation of the blood contained in them; neither is it a change in the reticular membrane, nor in the blood-vessels, because such contractions are not general throughout those substances. The coagulation of the blood

is a certain criterion of death. The reiterated visitations of blood are not essential to muscular irritability, because the limbs of animals, separated from the body, continue for a long time afterwards capable of contractions and relaxations.

The constituent elementary materials of which the peculiar animal and vegetable substances consist, are not separable by any chemical processes hitherto instituted, in such manner as to allow of a recombination into their former state. The composition of these substances appears to be naturally of transient duration, and the attractions of the elementary materials which form the gross substances are so loose and unsettled, that they are all decomposed without the intervention of any agent, merely by the operation of their own elementary parts on each other.

An extensive discussion of the chemical properties attaching to the matter of muscle would be a labour unsuited to this occasion: I should not, however, discharge my present duty, if I omitted to say that all such investigations can only be profitable when effected by simple processes, and when made upon the raw materials of the animal fabric, such, perhaps, as the albumen of eggs, and the blood. But until, by synthetical experiments, the peculiar substances of animals are composed from what are considered to be elementary materials, or the changes of organic secretion imitated by art, it cannot be hoped that any determinate knowledge should be established upon which the physiology of muscles may be explained. Such researches and investigations promise, however, the most probable ultimate success; since the phænomena are nearest allied to those of chemistry, and since all other hypotheses have, in their turns, proved unsatisfactory.

Facts and Experiments tending to support and illustrate the preceding Argument.

An emaciated horse was killed by dividing the medulla spinalis, and the large blood-vessels under the first bone of the sternum.

The temperature of the	flowing blood was	103°
	Spleen -	103
	Stomach -	101
	Colon -	98
	Bladder of urine	97
	Atmosphere -	30

Three pigs, killed by a blow on the head, and by the immediate division of the large arteries and veins, entering the

the middle of the basis of the heart, had the blood flowing from these vessels of 106, 106½, and 107°; the atmospheric temperature being at 31°.

An ox, killed in a similar manner, the blood 103°; atmosphere 50°.

Three sheep, killed by dividing the carotid arteries, and internal jugular veins: their blood 105, 105, 105½°; atmosphere 41°.

Three frogs, kept for many days in an equable atmosphere at 54°; their stomachs 62°.

The watery fluid issuing from a person tapped for dropsy of the belly 101°; the atmosphere being 43°, and the temperature of the superficies of the body at 96°.

These temperatures are considerably higher than the common estimation.

A man's arm being introduced within a glass cylinder, it was duly closed at the end which embraced the head of the humerus; the vessel being inverted, water at 97° was poured in, so as to fill it. A ground brass plate closed the lower aperture, and a barometer tube communicated with the water at the bottom of the cylinder. This apparatus, including the arm, was again inverted, so that the barometer tube became a gage, and no air was suffered to remain in the apparatus. On the slightest action with the muscles of the hand, or fore-arm, the water ascended rapidly in the gage, making librations of six and eight inches length in the barometer tube on each contraction and relaxation of the muscles.

The remarkable effects of crimping fish by immersion in water, after the usual signs of life have disappeared, are worthy of attention; and whenever the rigid contractions of death have not taken place, this process may be practised with success. The sea fish destined for crimping are usually struck on the head when caught, which, it is said, protracts the term of this capability; and the muscles which retain this property longest are those about the head. Many transverse sections of the muscles being made, and the fish immersed in cold water, the contractions called crimping take place in about five minutes; but, if the mass be large, it often requires thirty minutes to complete the process.

Two flounders, each weighing 1926 grains, the one being in a state for crimping, the other dead and rigid, were put into water at 48°, each being equally scored with a knife. After half an hour the crimped fish had gained in weight

53 grains,

53 grains, but the dead fish had lost 7 grains. The specific gravity of the crimped fish was greater than that of the dead fish; but a quantity of air bubbles adhered to the surfaces of the crimped muscles, which were rubbed off before weighing: this gas was not inflammable.

The specific gravity of the crimped fish -	1·105
of the dead fish, after an equal immersion in water	1·090

So that the accession of water, specifically lighter than the muscle of fish, did not diminish the specific gravity of crimped muscle, but the contrary: a proof that condensation had taken place.

A piece of cod fish, weighing twelve pounds, gained in weight, by crimping, two ounces avoirdupois; and another less vivacious piece, of fifteen pounds, gained one ounce and half*.

The hinder limb of a frog, having the skin stripped off, and weighing $77\frac{1}{10}$ grains, was immersed in water at 54°, and suffered to remain nineteen hours, when it had become rigid, and weighed $100\frac{1}{4}$ grains. The specific gravity of the contracted limb had increased, as in the crimped fish.

630 grains weight of the subscapularis muscle of a calf, which had been killed two days from the 10th of January, was immersed in New River water at 45°. After ninety minutes the muscle was contracted, and weighed in air 770 grains: it had also increased in specific gravity, but the quantity of air bubbles formed in the interstitial spaces of the reticular membrane made it difficult to ascertain the degree.

Some of the smallest fasciculi of muscular fibres from the same veal, which had not been immersed in water, were placed on a glass plate, in the field of a powerful microscope, and a drop of water thrown over them, at the temperature of 54°, the atmosphere in the room being 57°. They instantly began to contract, and became tortuous.

On confining the ends of another fibril with little weights of glass, it contracted two-thirds of its former length, by similar treatment. The same experiment was made on the muscular fibres of lamb and beef, twelve hours after the animals had been killed, with the like results. Neither vi-

* I am informed that the crimping of fresh water fishes requires hard water, or such as does not suit the purposes of washing with soap. This fact is substantiated by the practice of the London fishmongers, whose experience has taught them to employ pump water, or what is commonly called hard water.

negar,

negar, nor water saturated with muriate of soda, nor strong ardent spirit, nor olive oil, had any such effect upon the muscular fibres.

The amphibia, and coleopterous insects, become torpid at 34°: at 36° they move slowly, and with difficulty; and at a lower temperature their muscles cease to be irritable. The muscles of warm-blooded animals are similarly affected by cold.

The hinder limbs of a frog were skinned, and exposed to cold at 30°, and the muscles were kept frozen for eight hours; but on thawing them they were perfectly irritable.

The same process was employed in the temperature of 20°, and the muscles kept frozen for twelve hours; but that did not destroy the irritability.

In the heat of 100°, the muscles of cold-blooded animals fall into the contractions of death; and at 110°, all those of warm blood, as far as these experiments have been extended. The muscles of warm-blooded animals, which always contain more red particles in their substance than those of cold blood, are soon deprived of their irritability, even although their relative temperatures are preserved; and respiration in the former tribe is more essential to life than in the latter.

Many substances accelerate the cessation of irritability in muscles when applied to their naked fibrils, such as all the narcotic vegetable poisons, muriate of soda, and the bile of animals; but they do not produce any other apparent change in muscles than that of the last contraction. Discharges of electricity passed through muscles, destroy their irritability, but leave them apparently inflated with small bubbles of gas; perhaps some combination obtains which decomposes the water.

The four separated limbs of a recent frog were skinned, and immersed in different fluids; viz. No. 1. in a phial containing six ounces, by measure, of a saturated aqueous solution of liver of sulphur made with potash; No. 2. in a diluted acetic acid, consisting of one drachm of concentrated acid to six of water; No. 3. in a diluted alkali, composed of caustic vegetable alkali, one drachm, of water six ounces; No. 4. in pure distilled water.

The phials were all corked, and the temperature of their contents was 46°.

The limb contained in the phial No. 1, after remaining twenty minutes, had acquired a pale red colour, and the muscles were highly irritable.

The limb in No. 2, after the same duration, had become rigid, white, and swollen; it was not at all irritable. By

 removing

removing the limb into a diluted solution of vegetable alkali, the muscles were relaxed, but no signs of irritability returned.

No. 3. under all the former circumstances, retained its previous appearances, and was irritable, but less so than No. 1.

No. 4. had become rigid, and the final contraction had taken place.

Other causes of the loss of muscular irritability occur in pathological testimonies, some examples of which may not be ineligible for the present subject. Workmen whose hands are unavoidably exposed to the contact of white lead, are liable to what is called a palsy in the hands and wrists, from a torpidity of the muscles of the fore arm. This affection seems to be decidedly local, because, in many instances, neither the brain nor the other members partake of the disorder; and it oftenest affects the right hand. An ingenious practical chemist in London has frequently experienced spasms and rigidity in the muscles of his fore arms, from affusions of nitric acid over the cuticle of the hand and arm. The use of mercury occasionally brings on a similar rigidity in the masseter muscles.

A smaller quantity of blood flows through a muscle during the state of contraction than during the quiescent state, as is evinced by the pale colour of red muscles when contracted. The retardation of the flow of blood from the veins of the fore arm, during venæsection, when the muscles of the limb are kept rigid, and the increased flow after alternate relaxations, induces the probability, that a temporary retardation of the blood in the muscular fibrils takes place during each contraction, and that its free course obtains again during the relaxation. This state of the vascular system in a contracted muscle, does not, however, explain the diminution of its bulk, although it may have some influence on the limb of a living animal.

When muscles are vigorously contracted, their sensibility to pain is nearly destroyed: this means is employed by jugglers for the purpose of suffering pins to be thrust into the calf of the leg, and other muscular parts, with impunity: it is, indeed, reasonable to expect, *à priori*, that the sensation, and the voluntary influence, cannot pass along the nerves at the same time.

In addition to the influences already enumerated, the human muscles are susceptible of changes from extraordinary occurrences of sensible impressions. Long continued attention to interesting visible objects, or to audible sensations,

tions, are known to exhaust the muscular strength: intense thought and anxiety weaken the muscular powers, and the passions of grief and fear produce the same effect suddenly; whilst the contrary feelings, such as the prospect of immediate enjoyment, or moderate hilarity, give more than ordinary vigour.

It is a very remarkable fact in the history of animal nature, that the mental operations may become almost automatic, and, under such habit, be kept in action, without any interval of rest, far beyond the time which the ordinary state of health permits, as in the examples of certain maniacs, who are enabled, without any inconvenience, to exert both mind and body for many days incessantly. The habits of particular modes of labour and exercise are soon acquired, after which the actions become automatic, demand little attention, cease to be irksome, and are affected with little fatigue: by this happy provision of nature, the habit of industry becomes a source of pleasure, and the same appears to be extended to the docile animals which co-operate with man in his labours.

Three classes of muscles are found in the more complicated animals. Those which are constantly governed by the will, or directing power of the mind, are called voluntary muscles. Another class, which operate without the consciousness of the mind, are denominated involuntary: and a mixed kind occur in the example of respiratory muscles, which are governed by the will to a limited extent: nevertheless the exigencies of the animal feelings eventually urge the respiratory movements in despite of the will. These last muscles appear to have become automatic by the continuance of habit.

The uses of voluntary muscles are attained by experience, imitation, and instruction; but some of them are never called into action among Europeans, as the muscles of the external ears, and generally the occipito-frontalis. The purely involuntary muscles are each acted upon by different substances, which appear to be their peculiar stimuli; and these stimuli co-operate with the sensorial influence in producing their contractions: for example, the bile appears to be the appropriate stimulus of the muscular fibres of the alimentary canal below the stomach, because the absence of it renders those passages torpid. The digested aliment, or perhaps the gastric juice in a certain state, excites the stomach. The blood stimulates the heart, light the iris of the eye, and mechanical pressure seems to excite the muscles of the œsophagus. The last cause may perhaps be il-

 lustrated

lustrated by the instances of compression upon the voluntary muscles, when partially contracted, of which there are many familiar examples. Probably the muscles of the ossicula auditûs are awakened by the tremors of sound; and this may be the occasion of the peculiar arrangement observable in the chorda tympani which serves those muscles.

These extraneous stimuli seem only to act in conjunction with the sensorial power derived by those muscles from the gangliated nerves, because the passions of the mind alter the muscular actions of the heart, the alimentary canal, the respiratory muscles, and the iris; so that, probably, the respective stimuli already enumerated, only act subserviently, by awakening the attention of the sensorial power, (if that expression may be allowed,) and thereby calling forth the nervous influence, which, from the peculiar organization of the great chain of sympathetic nerves, is effected without consciousness; for, when the attention of the mind, or the more interesting passions prevail, all the involuntary muscles act irregularly and unsteadily, or wholly cease. The movements of the iris of the common parrot are a striking example of the mixed influence.

The muscles of the lower tribes of animals, which are often entirely supplied by nerves coming from ganglions, appear of this class; and thus the animal motions are principally regulated by the external stimuli, of which the occurrence seems to agree with the animal necessities: but the extensive illustrations which comparative anatomy affords on this point, are much too copious for any detail in this place.

There are two states of muscles; one active, which is that of contraction; the other, a state of ordinary tone, or relaxation, which may be considered passive, as far as it relates to the mind; but the sensorial or nervous power seems never to be quiescent, as it respects either the voluntary or involuntary muscles during life. The yielding of the sphincters appears to depend on their being overpowered by antagonist muscles, rather than on voluntary relaxation, as is commonly supposed.

I have now finished this endeavour to exhibit the more recent historical facts connected with muscular motion.

It will be obvious to every one, that much remains to be done before any adequate theory can be proposed. I have borrowed from the labours of others without acknowledgment, because it would be tedious to trace every fact, and every opinion, to its proper authority; many of the views are perhaps peculiar to myself, and I have adduced many

4 general

general assumptions and conclusions, without offering the particular evidence for their confirmation, from a desire to keep in view the remembrance of retrospective accounts, and to combine them with intimations for future research. The due cultivation of this interesting pursuit cannot fail to elucidate many of the phænomena in question, to remove premature and ill-founded physiological opinions, and eventually to aid in rendering the medical art more beneficial, by establishing its doctrines on more extensive and accurate views of the animal œconomy.

XLIV. *Information on the Mines and Manufactures of the East Indies, and other Subjects. By* J. MACHLACHLAN, *Esq. of Calcutta**.

SIR,

SHOULD you think the enclosed receipts for dyeing the beautiful reds of the Coromandel coast can be of any use to the dyers of the united British kingdom, be pleased to lay them before the Society for the Encouragement of Arts, &c. that they may be published in the volume of their Transactions; if not, I trust you will excuse my troubling you with them. They were sent to me from Madras by a scientific friend, who had the several operations, detailed in them, performed in his own presence. I forwarded a copy of them, and a small quantity of the ingredients mentioned in them, to a friend at home, several years ago; but he dying about or soon after the time of their arrival, I never learned what became of them. It strikes me, however, that there is a considerable coincidence between the thread process and that which I have seen recommended by Mr. Henry, of Manchester, for dyeing the Adrianople or Turkey red.

I am not certain whether it is known at home, that many of the hills in Bahar, and other parts of India, contain immense quantities of mica, talc, or Muscovy glass. The natives of this country and China make very splendid lanterns, shades, and ornaments of it, tinged of various fanciful colours; and it is also used by them in medicine. When burned or calcined, it is, I am told, considered as a specific in obstinate coughs and consumptions. When powdered it serves to silver the Indian paper, &c. used in

* From *Transactions of the Society of Arts*, &c. 1804, which voted its silver medal to the author for the communication.

 letter-

letter-writing; and, in fact, it is applied to numberless purposes. The bazar price of that of the best quality, split into sheets of about two lines thick, is six rupees the maund of eighty-four pounds avoirdupois. If it could be applied to any useful purpose at home, it might go in part ballast of ships, and at a trifling expense. I enclose a small specimen of it, and am, sir,

Your very obedient servant,

Calcutta, Oct. 4, 1803. J. MACHLACHLAN.

N. B. The chaya, or red dye root of the coast, is, I believe, known at home: as also the cashan leaves, which are used as an astringent.

Charles Taylor, Esq.

Directions for dyeing a bright Red, four Yards of three-quarters broad Cotton Cloth.

1st, The cloth is to be well washed and dried, for the purpose of clearing it of lime and congee, or starch, generally used in India for bleaching and dressing cloths; then put into an earthen vessel, containing twelve ounces of chaya or red dye root, with a gallon of water, and allow it to boil a short time over the fire.

2d, The cloth being taken out, washed in clean water, and dried in the sun, is again put into a pot with one ounce of myrabolans, or galls coarsely powdered, and a gallon of clear water, and allowed to boil to one half; when cool, add to the mixture a quarter of a pint of buffalo's milk. The cloth being fully soaked in this, take it out, and dry it in the sun.

3d, Wash the cloth again in clear cold water, and dry it in the sun; then immerse it into a gallon of water, a quarter of a pint of buffalo's milk, and a quarter of an ounce of the powdered galls. Soak well in this mixture, and dry in the sun. The cloth, at this stage of the process, feeling rough and hard, is to be rolled up and beetled till it becomes soft.

4th, Infuse into six quarts of cold water six ounces of red wood shavings, and allow it to remain so two days. On the third day boil it down to two-thirds the quantity, when the liquor will appear of a good bright red colour. To every quart of this, before it cools, add a quarter of an ounce of powdered alum; soak in it your cloth twice over, drying it between each time in the shade.

5th, After three days wash in clean water, and half dry in the sun; then immerse the cloth into five gallons of water,

water, at about the temperature of one hundred and twenty degrees of Fahrenheit, adding fifty ounces of powdered chaya, and allowing the whole to boil for three hours; take the pot off the fire, but let the cloth remain in it until the liquor is perfectly cool; then wring it gently, and hang it up in the sun to dry.

6th, Mix intimately together, by hand, about a pint measure of fresh sheep's dung, with a gallon of cold water, in which soak the cloth thoroughly, and immediately take it out, and dry it in the sun.

7th, Wash the cloth well in clean water, and spread it out in the sun on a sand-bank (which in India is universally preferred to a grass-plat) for six hours, sprinkling it from time to time, as it dries, with clean water, for the purpose of finishing and perfecting the colour, which will be of a very fine bright red.

Calcutta, Oct. 4, 1803. J. MACHLACHLAN.

Charles Taylor, Esq.

Directions for dyeing of a beautiful Red, eight Ounces of Cotton Thread.

1st, Put one gallon and a half, by measure, of sap-wood ashes, into an earthen pot, with three gallons of water, and allow the mixture to remain twenty-four hours to perfect it for use.

2d, Put the following articles into an earthen pot, viz. Three-quarters of a pint of Gingelly oil; one pint, by measure, of sheep's dung, intimately mixed by hand in water; two pints of the above ley. After mixing these ingredients well, pour the mixture gradually upon the thread into another vessel, wetting it only as the thread, by being squeezed and rolled about by the hand, imbibes it, continuing to do so until the whole is completely soaked up, and allow the thread to remain in this state until next day.

3d, Take it up, and put it in the sun to dry; then take a pint and a half of ash-ley, in which squeeze and roll the thread well, and allow it to remain till next day.

4th, Squeeze and roll it in a like quantity of ash-ley, and put it in the sun to dry; when dry, squeeze and roll it again in the ley, and allow it to remain till next day.

5th, Let the same process be repeated three or four times, and intermit till next day.

6th, Ley the thread once, as the day before, and, when well dried in the sun, prepare the following liquor: One gill of Gingelly oil; one pint and a half of ash-ley.—In

this squeeze and roll the thread well, and leave it so till next day.

7th, Repeat the process of yesterday, and dry the thread in the sun.

8th, The same process to be repeated.

9th, First repeat the ash-ley process three or four times, as under the operations 3, 4, and 5, and then prepare the following mixture: One pint of sheep-dung water; one gill of Gingelly oil; one pint and a half of ash-ley.—In this squeeze and roll the thread well, and dry it in the sun.

10th, Repeat the same process.
11th, Do. do.
12th, Do. do.
13th, Do. do.
14th, Do. do.

15th, Wash the thread in clean water, and squeeze and roll it in a cloth until almost dry; then put it into a vessel containing a gill of powdered chaya root, one pint by measure of cashan leaves, and ten pints of clear water; in this liquor squeeze and roll it about well, and allow it to remain so till next day.

16th, Wring the thread, and dry it in the sun, and repeat again the whole of the 15th process, leaving the thread to steep.

17th, Wring it well, dry it in the sun, and repeat the same process as the day before.
18th, Do. do.
19th, Do. do.

20th, Wring and dry it in the sun, and with the like quantity of chaya root in ten pints of water boil the thread for three hours, and allow it to remain in the infusion until cold.

21st, Wash the thread well in clear water, dry it in the sun, and the whole process is complete.

Calcutta, Oct. 4, 1803. J. MACHLACHLAN.

XLV. *On the Direction and Velocity of the Motion of the Sun and Solar System.* By WILLIAM HERSCHEL, *LL.D. F.R.S.**

OUR attention has lately been directed again to the construction of the heavens, on which I have already delivered

* From the *Transactions of the Royal Society* for 1805.

several

several detached papers. The changes which have taken place in the relative position of double stars, have ascertained motions in many of them, which are probably of the same nature with those that have hitherto been called proper motions. It is well known that many of the principal stars have been found to have changed their situation, and we have lately had a most valuable acquisition in Dr. Maskelyne's table of proper motions of six-and-thirty of them. If this table affords us a proof of the motion of the stars of the first brightness, such as are probably in our immediate neighbourhood, the changes of the position of minute double stars that I have ascertained, many of which can only be seen by the best telescopes, likewise prove that motions are equally carried on in the remotest parts of space which hitherto we have been able to penetrate.

The proper motions of the stars have long engaged the attention of astronomers, and in the year 1783 I deduced from them, with a high degree of probability, a motion of the sun and solar system towards λ Herculis. The reasons which were then pointed out for introducing a solar motion, will now be much strengthened by additional considerations; and the above-mentioned table of well-ascertained proper motions will also enable us to enter rigorously into the necessary calculations for ascertaining its direction, and discovering its velocity. When these points are established, we shall be prepared to draw some consequences from them that will account for many phænomena which otherwise cannot be explained.

The scope of this paper, wherein it is intended to assign not only the direction, but also the velocity of the solar motion, embraces an extensive field of observation and calculation; but as to give the whole of it would exceed the compass of the present sheets, I shall reserve the velocity of the solar motion for an early future opportunity, and proceed now to a disquisition of the first part of my subject, which is the direction of the motion of the sun and solar system.

Reasons for admitting a Solar Motion.

It may appear singular that, after having already long ago pointed out a solar motion, and even fixed upon a star towards which I supposed it to be directed, I should again think it necessary to show that we have many substantial reasons for admitting such a motion at all. What has induced me to enter into this inquiry is, that some of the

consequences hereafter to be drawn from a solar motion, when established, seem to contradict the very intention for which it is to be introduced. The chief object in view, when a solar motion was proposed to be deduced from observations of the proper motions of stars, was to take away many of these motions by investing the sun with a contrary one. But the solar motion, when its existence has been proved, will reveal so many concealed real motions, that we shall have a greater sum of them than it would be necessary to admit, if the sun were at rest; and, to remove this objection, the necessity for admitting its motion ought to be well established.

Theoretical Considerations.

A view of the motion of the moons, or secondary planets, round their primary ones, and of these again round the sun, may suggest the idea of an additional motion of the latter round some other unknown centre; and those who like to indulge in fanciful reviews of the heavens, might easily build a system upon hypotheses not altogether without some plausibility in their favour. Accordingly we find that Mr. Lambert, in a work which is full of the most fantastic imaginations, has framed a system wherein the sun is supposed to move about the nebula in Orion*. But, setting aside the extravagant idea of making this luminous spot a centre of motion, it must certainly be admitted that the solar motion itself is at least a very possible event.

I have already mentioned, in a note to my former paper†, that the possibility of a solar motion has also been shown from theoretical principles by the late Dr. Wilson, of Glasgow; and its probability afterwards, from reasons of the same nature, by Mr. de la Lande. The rotatory motion of the sun, from which he concludes a displacing of the solar centre, must certainly be allowed to indicate a motion of translation in space; for though it may be possible, it does not appear probable, that any mechanical impression should have given the former, without occasioning the latter. But, as we are entirely unacquainted with the cause of the rotatory motion, the solar translation in space from theoretical reasons, can only be admitted as a very plausible hypothesis.

It would be worth while for those who have fixed instruments, to strengthen this argument by observing the stars

* See *Système du Monde* de Mr. Lambert, p. 152 and 158.

† See Phil. Trans. for the year 1783, p. 283.

which

which are known to change their magnitudes periodically. For, as we have great reason to ascribe these regular changes to a rotatory motion of the stars*, a real motion in space may be expected to attend it; and the number of these stars is so considerable, that their concurring testimony would be very desirable.

Perhaps Algol, which according to these ideas must have a very quick rotatory motion, may be found to have also a considerable progressive one; and if that should be ascertained, the position of the axis of the rotation of this star will be in a great measure thereby discovered.

An argument from the real motion to a rotatory one is nearly of equal validity, and therefore all the stars that have a motion in space may be surmised to have also a rotation on their axes.

Symptoms of parallactic Motions.

But, setting aside theoretical arguments, I shall now proceed to such as may be drawn from observation; and as all parallactic motions are evident indications that the observer of them is not at rest, it will be necessary to explain three sorts of motions, of which the parallactic is one; they will often engage our attention in the following discussion.

Let the sun be supposed to move towards a certain part of the heavens; and since the whole solar system will have the same motion, the stars must appear to an inhabitant of the earth to move in an opposite direction. In the triangle $s\ p\ a$, (Plate VIII.) Fig. 1, let $s\ p$ represent the parallactic motion of a star; then, if this star is one that has no real motion, $s\ p$ will also be its apparent motion; but if the star in the same time, that by its parallactic motion it would have gone from s to p, should have a real motion which would have carried it from s to r, then will it be seen to move along the diagonal $s\ a$, of the parallelogram $s\ r\ p\ a$; and $p\ a$, which is parallel and equal to $s\ r$, will represent its real motion. Therefore, in the above-mentioned triangle $s\ p\ a$, which I suppose to be formed in the concave part of the heavens by three arches of great circles, the eye of the observer being in the centre, the three sides will represent, or stand for, the three motions I have named: $s\ p$ the parallactic, $p\ a$ the real, and $s\ a$ the apparent motion of the star. The situation and length of these arches, in seconds of a degree, will express, or rather represent, not only the direction but also the quantity of each motion, such as it

* See Phil. Trans. for the year 1795, p 68.

must

must appear to an eye in the above-mentioned central situation. And calling the solar motion S, the distance of the star from the sun d, and the sine of the star's distance from the point towards which the sun is moving ϕ, the parallactic motion, when these are given, will be had by the expression $\frac{\phi . S}{r . d} = s\ p$. This theorem, and its corollaries, of which frequent use will be made hereafter, it will not be necessary here to demonstrate.

When I call the arch $p\ u$ the real motion, it should be understood that I only mean its representative; for it must be evident that the absolute motion of a star in space, as well as its intrinsic velocity, will still remain unknown, because the inclination of that motion on which also its real velocity will depend, admits of the greatest variety of directions. We are only acquainted with the plane in which the motion must be performed, and with the length of the arch in seconds by which that motion may be measured. We may add that the chords of the arches representing the three motions are the smallest velocities of these motions that can be admitted; for in every other direction but at right angles to the line of sight, the actual space over which the star will move must be greater than the arch or chord by which its motion is represented.

Now, since a motion of the sun will occasion parallactic motions of the stars, it follows that these again must indicate a solar motion; but in order to ascertain whether parallactic motions exist, we ought to examine those stars which are most liable to be visibly affected by solar motion. This requisite points out the brightest stars as the most proper for our purpose; for any star may have a great real motion, but in order to have a great parallactic one, it must be in the neighbourhood of the sun. And as we can only judge of the distance of the stars by their splendour, we ought to choose the brightest, on account of a probability that, being nearer than faint ones, they may be more within the reach of parallax, and thus better qualified to show its effects.

We are also to look out for a criterion whereby parallactic may be distinguished from real motions; and this we find in their directions. For, if a solar motion exists, all parallactic motions will tend to a point in opposition to the direction of that motion; whereas real motions will be dispersed indiscriminately to all parts of space.

With these distinctions in view, we may examine the pro-

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per motions of the principal stars; for these, if the sun is not at rest, must either be entirely parallactic, or at least composed of real and parallactic motions; in the latter case they will fall under the denomination of one of the three motions we have defined, namely *s a*, the apparent motion of the star.

In consequence of this principle I have delineated the meeting of the arches arising from a calculation of the proper motions of the 36 stars in Dr. Maskelyne's catalogue, on a celestial globe; and, as all great circles of a sphere intersect each other in two opposite points, it will be necessary to distinguish them both: for, if the sun moves to one of them, it may be called the apex of its motion; and as the stars will then have a parallactic motion to the opposite one, the appellation of a parallactic centre may very properly be given to it. The latter falling into the southern hemisphere, among constellations not visible to us, I shall only mention their opposite intersections; and of these I find no less than ten that are made by stars of the first magnitude, in a very limited part of the heavens, about the constellation of Hercules. Upon all the remaining surface of the same globe there is not the least appearance of any other than a promiscuous situation of intersections; and of these only a single one is made by arches of principal stars.

The ten intersecting points made by the brightest stars are as follows: The 1st is by Sirius and Arcturus, in the mouth of the Dragon. The 2d by Sirius and Capella, near the following hand of Hercules. The 3d by Sirius and Lyra, between the hand and knee of Hercules. The 4th by Sirius and Aldebaran, in the following leg of Hercules. The 5th by Arcturus and Capella, north of the preceding wing of the Swan. The 6th by Arcturus and Aldebaran, in the neck of the Dragon. The 7th by Arcturus and Procyon, in the preceding foot of Hercules. The 8th by Capella and Procyon, south of the following hand of Hercules. The 9th by Lyra and Procyon, preceding the following shoulder of Hercules. And the 10th is made by Aldebaran and Procyon, in the breast of Hercules.

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The following Table gives the calculated situation of these ten intersections in right ascension and north polar distance.

Table I.

No.	Right Ascension.			Polar Distance.		
1	255°	39′	50″	36°	41′	34″
2	275	9	32	64	21	48
3	272	23	58	58	23	24
4	263	25	38	44	39	47
5	290	0	58	32	7	23
6	267	2	19	33	57	20
7	235	3	13	46	21	34
8	272	51	49	73	7	56
9	266	46	49	66	48	11
10	260	1	29	60	59	34

We might rest satisfied with having shown that the parallactic effect of which we are in search is plainly to be perceived in the motion of the brightest stars; however, by way of further confirmation, we may take in some large stars of the next order, in whose motions evident marks of the influence of parallax may likewise be perceived. When the intersections made by their proper motions and the arches in which the stars of the first magnitude are moving are examined, we find no less than fifteen which unite with the former ten, in pointing out the same part of the heavens as a parallactic centre. It will be sufficient only to mention the opposite points of the situation of these intersections, and the stars by which they are made, without giving a calculated table of them.

The 1st is the following leg of Hercules, and is made by Sirius and β Tauri. The 2d is also in the following leg of Hercules, by Sirius and α Andromedæ. The 3d is in the following hand of Hercules, by Sirius and α Arietis. The 4th in the neck of the Dragon, by Arcturus and β Tauri. The 5th between the Lyre and the northern wing of the Swan, by Capella and α Andromedæ. The 6th near the following hand of Hercules, by Capella and α Arietis. The 7th preceding the head of Hercules, by Lyra and β Tauri. The 8th between the Lyre and northern wing of the Swan, by Lyra and α Andromedæ. The 9th in the following arm

arm of Hercules, by Lyra and α Arietis. The 10th in the following leg of Hercules, by Aldebaran and β Tauri. The 11th in the following leg of Hercules, by Aldebaran and α Andromedæ. The 12th in the head of Hercules, by Aldebaran and α Arietis. The 13th in the following arm of Hercules, by Procyon and β Tauri. The 14th in the back of Hercules, by Procyon and α Andromedæ. And the 15th near the following arm of Hercules, is made by Procyon and α Arietis.

An argument like this, founded upon the most authentic observations, and supported by the strictest calculations, can hardly fail of being convincing. And though only the ten principal apices of the twenty-five that are given have been calculated, the other fifteen may nevertheless be depended upon as true to less than one degree of the sphere.

Changes in the Position of double Stars.

We have lately seen that the alterations in the relative situation of a great number of double stars may be accounted for by a parallactic motion. Among the 56 stars which I have given, the changes of more than half of them appear to be of this nature; and it will certainly be more eligible to ascribe them to the effect of parallax than to admit so many separate motions in the different stars; especially when it is considered, that if the alterations of the angle of position were owing to a motion of the largest star of each set, the direction of such motions must, in contradiction to all probability, tend nearly to one particular part of the heavens.

This argument, drawn from the change of the position of double stars, may be considered as deriving its validity from the same source with the former, namely, the parallactic motions of at least 28 more stars, pointing out the same apex of a solar motion by their direction to its opposite parallactic centre.

Incongruity of proper Motions.

It may be remarked that the proper motions of the stars, if they were in reality such as they appear to be, would contain a certain incongruous mixture of great velocity and extreme slowness. Arcturus alone describes annually an arch of more than two seconds: Aldebaran hardly one-tenth and a quarter of a second: Rigel little more than one-tenth and a half: even Lyra moves barely three and a quarter tenths of a second, while Procyon has almost four times that velocity. Out of 36 stars, whose proper motion we

have examined, there are 15 that do not reach two-tenths of a second: β Virginis moves seventy-seven hundredths, and α Cygni only six. But it will be shown, when the direction and velocity of the solar motion come to be explained, that these kind of incongruities are mere parallactic appearances; and that there is so general a consistency among the real motions of the stars, that Arcturus is in no respect singled out as a star whose motion is far beyond the rest.

By giving this remark a place among the reasons for admitting a solar motion, it is not intended to lay any particular stress upon it; for it may be objected that our idea of the congruence or harmony of the celestial motions can be no criterion of their real fitness and symmetry. But when such discordant proper motions as those I have mentioned in stars of no very different lustre are under consideration, and may be easily shown to be only parallactic phænomena, the method by which this can be done must certainly appear eligible, and, when added to many other inducements, will throw some share of weight into the scale.

Sidereal Occultation of a small Star.

Of nearly the same importance with the former argument is the account of the occultation of a small star by a large one, which I have given in my last paper. When the solar motion has been established, we shall prove that the vanishing of the small star near δ Cygni, as far as we can judge at present, is only a parallactic disappearance. It must be granted that a real motion of the large star would also explain the same phænomenon; but then again, this star must be supposed to move towards the very same parallactic centre which the changes in the position of other double stars point out; and this cannot be probable.

Direction of the Solar System.

From what has been said, I believe the expedience of admitting a solar motion will not be called in question; our next endeavour therefore must be to investigate its direction.

To return to the before-mentioned intersections of the arches, in which the proper motions of the stars are performed, I shall begin by proving that when the proper motions of two stars are given, an apex may be found, to which, if the sun be supposed to move with a certain velocity, the two given motions may then be resolved into apparent changes,

changes, arising from sidereal parallax, the stars remaining perfectly at rest.

Let the stars be Arcturus and Sirius, and their annual proper motions as given in the Astronomer Royal's Tables.

When the annual proper motion of Arcturus, which is $-1'',26$ in right ascension, and $+1'',72$ in north polar distance, is reduced by a composition of motions to a single one, it will be in a direction which makes an angle of $55° 29' 42''$ south-preceding with the parallel of Arcturus, and of a velocity so as to describe annually $2'',08718$ of a great circle.

The annual proper motion of Sirius, $-0'',42$ in right ascension, and $+1'',04$ in north polar distance, by the same method of composition, becomes a motion of $1'',11528$, in a direction which makes an angle of $68° 49' 41''$ south-preceding with the parallel of Sirius.

By calculation, the arches in which these two stars move, when continued, will meet in what I have called their parallactic centre, whose right ascension is $75° 39' 50''$, and south polar distance is $36° 41' 34''$. The opposite of this, or right ascension $255° 39' 50''$, and north polar distance $36° 41' 34''$, is what we are to assume for the required apex of the solar motion.

When a star is situated at a certain distance from the sun, which we shall call 1; and 90° from the apex of the solar motion, its parallactic motion will be a maximum. Let us now suppose the velocity of the sun to be such that its motion, to a person situated on this star, would appear to describe annually an arch of $2'',84825$, or, which is the same thing, that the star would appear to us, from the effect of parallax, to move over the above-mentioned arch in the same time.

To apply this to Arcturus, we find by calculation that its distance from the apex of the solar motion is $47° 7' 6''$; its parallactic motion therefore, which is as the sine of that distance, will be $2'',08718$; and this, as has been shown, is the apparent motion which observation has established as the proper motion of Arcturus.

In the next place, if we admit Sirius to be a very large star situated at the distance 1,6809 from us, and compute its elongation from the apex of the solar motion, we shall find it $138° 50' 14'',5$. With these two data we calculate that its parallactic motion will be $\frac{\phi \, . \, S}{r \, . \, d} = s p = 1'',11528$; and this also agrees with the apparent motion which has been

been ascertained by observation as the proper motion of Sirius.

Now since, according to the rules of philosophizing, we ought not to admit more motions than will account for the observed changes in the situation of the stars, it would be wrong to have recourse to the motions of Arcturus and Sirius, when that of the sun alone will account for them both; and this consideration would be a sufficient inducement for us to fix at once on the calculated apex, as well as on the relative distances that have been assigned to these stars, if other proper motions could with equal facility be resolved into similar parallactic appearances. But from the nature of proper motions, it follows, that when a third star does not lead us to the same apex as the other two, its apparent motion cannot be resolved by the effect of parallax alone. And to enhance our difficulties, the number of apices, that would be required to solve all proper motions into parallactic ones, increases not as the number of stars admitted to have proper motions, but, when their situation happens to be favourable, as the sum of an arithmetical series of natural numbers, beginning at 0, continued to as many terms as there are stars admitted: so that if two stars give only one apex, one star added to it will give three apices; and ten, for instance, will give no less than 45, and so on.

The method of reasoning which, on this subject, I have adopted, is so closely connected with astronomical observations that I shall keep them constantly in view; and therefore shall illustrate what has been advanced, by taking in Capella as a third star. The three apices which then are pointed out will be that in the mouth of the Dragon, by Arcturus and Sirius; a second under the northern wing of Cygnus, by Arcturus and Capella; and a third in the following hand of Hercules, by Sirius and Capella. The calculation of them is in Table I.

The annual proper motions of our third star in Dr. Maskelyne's Tables are $+0'',21$ in right ascension, and $+0'',44$ in north polar distance; and by calculation these quantities give an annual motion of $0'',16374$ to Capella, in a direction which makes an angle of $71^\circ\ 35'\ 22'',4$ south-following with the parallel of this star.

The distance of Capella from the same calculated apex of the solar motion, by which we have already explained the apparent motions of the other two stars, is $80^\circ\ 54'\ 46''$; and, admitting again the velocity of the sun towards the same point as stated before, it will occasion a parallactic motion

motion of Capella, in a direction 89° 54′ 48″ south-following its parallel, amounting to 2,8125″. In this calculation Capella has been taken for a star of the first magnitude, supposing its distance from us to be equal to that of Arcturus.

By constructing then a triangle, the three sides of which will represent the three motions which every star must have that is not at rest in space; we have one of the sides, representing the apparent motion of the star, equal to 0,4637″; the other side, being the parallactic motion of the star, 2,8125″; and the included angle 18° 19′ 27″. From these data we obtain the third side, representing the real motion of the star, which will be 2,3757″. By the given situation of this triangle with respect to the parallel of declination of Capella, the angle of the real motion will also be had, which is 86° 34′ 11″ north-following the parallel of this star. A composition of the parallactic and the real motion in the directions we have assigned, will produce the annual apparent motion which has been established by observation.

But to apply what has been said to our present purpose, it may be observed, that although we have accounted for the proper motion of our third star by retaining the same apex of the solar motion, which has given us an explanation of the apparent motions of the other two, yet in doing this we have been obliged to assign a great degree of real motion to Capella; and to this it may be objected, that we can have no authority to deprive Arcturus and Sirius of real motions, in order to give one of the same nature to our third star: and indeed to every star that has a proper motion which does not tend to the same parallactic centre as the motions of Arcturus and Sirius.

This objection is perfectly well founded, and I have given the above calculation on purpose to show that, when we are in search of an apex for the solar motion, it ought to be so fixed upon as to be equally favourable to every star which is proper for directing our choice. Hence a problem will arise, in our present case, how to find a point whose situation among three given apices shall be so that, if the sun's motion be directed towards it, there may be taken away the greatest quantity of proper motion possible from the given three stars. The intricacy of the problem is greater than at first it may appear, because by a change of the distance of the apex from any one of the stars, its parallactic motion, which is as the sine of that distance, will be affected; so that it is not the mere alteration of the angle

of direction, which is concerned. However, it will not be necessary to enter into a solution of the problem; for it must be very evident that a much more complex one would immediately succeed it, since three stars would certainly not be sufficient to direct us in our present endeavour to find the best situation of an apex for the solar motion; I shall therefore now leave these stars, and the apices pointed out by them, in order to proceed to a more general view of the subject.

We have already seen that the brightest stars are most proper for showing the effect of parallax, and that in our search after the direction of the solar motion, our aim must be to reduce the proper motions of the stars to their lowest quantities. The six principal stars, whose intersecting arches have been given, when their proper motions in right ascension and polar distance are brought into one direction, will have the apparent motions contained in the following Table.

Table II.

Names of the Stars.	Direction of the apparent Motions.	Quantities of the apparent Motions.
Sirius - -	68° 49′ 40,7″ S. preceding	1,11528″ per year
Arcturus -	55 29 42,0 S. preceding	2,08718
Capella -	71 35 22,4 S. following	0,46374
Lyra -	56 20 57,3 N. following	0,32435
Aldebaran	76 29 37,3 S. following	0,12341
Procyon -	50 2 24,5 S. preceding	1,23941
	Sum of the apparent motions	5,35337″

We must now recur to what has been said, when the construction of the triangle expressing the three motions of a star, that is not at rest, was explained; and, as we are to find out a solar motion which will require the least real motion in our six stars, an attention to this triangle will be of considerable use; for when the line *p a*, Fig. 1, which represents the real motion, is brought into the situation *m a*, where it is perpendicular to *s p*, the real motion which is required will then be a minimum. It also follows, from the construction of the same triangle, that if by the choice of an apex for the solar motion we can lessen the angle made at *s* by the lines *s p* and *s a*, we shall lessen the quantity of real motion required to bring the star from the parallactic line *s p m* to the observed position *a*.

It has already been shown, in the case of Sirius and Arcturus, that when two stars only are given, the line *s p* may

be made to coincide with the lines $s\,a$, of both the stars, whereby their real motions will be reduced to nothing. It has also been proved, by adding Capella to the former two, that when three stars are concerned, some real motion must be admitted in one of them. Now, since all parallactic motions are directed to the same centre, a single line may represent the direction of the effect of the parallax, not only of these three stars but of every star in the heavens. According to this theory, let the line s P or s S, in Fig. 2, stand for the direction of the parallactic motion of the stars; and as in the foregoing Table we have the angles of the apparent motion of six stars with the parallel of each star, we must now also compute the direction of the line s P or s S with the parallels of the same stars. This may be done as soon as an apex for the solar motion is fixed upon. The difference between these angles and the former will give the several parallactic angles P $s\,a$ or S $s\,a$, required for an investigation of the least quantity $m\,a$, belonging to every star.

For instance, let the point towards which we may suppose the sun to move, be λ Herculis; and calculating the required angles of the direction in which the effect of parallax will be exerted, with the six stars we have selected for the purpose of our investigation, we find them as in the following Table.

Table III.

Angles of the parallactic Motion with the Parallel.

Sirius - - -	32°	54′	8,5″	south-preceding.
Arcturus -	17	23	45,7	south-preceding.
Capella -	85	10	3,9	south-following.
Lyra - -	35	59	49,5	north-following.
Aldebaran -	71	21	35,4	south-following.
Procyon -	47	43	44,6	south preceding.

The difference between these parallactic and the former apparent angles, with the parallel of each star, will give the required angles for our second figure. They will be as follows:

Table IV.

Angles of the apparent with the parallactic Motion.

Sirius - -	35°	55′	32,2″	south-following.
Arcturus -	38	5	56,3	south-following.
Capella -	13	34	41,5	south-following.
Lyra - -	20	21	7,8	north-preceding.
Aldebaran -	5	8	1,9	south-preceding.
Procyon -	2	18	39,9	south-following.

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By these angles, with the assistance of the lines *sa*, whose lengths represent the annual quantity of the apparent motions as given in our former table, the figure No. 2 has been constructed. When the situation of these angles is regulated as in that figure, we may draw the several lines *ma* perpendicular to S P, and, by computation, their value and sum will be obtained as follows:

Table V.

Quantities and Sum of the least real Motions.

Sirius	-	-	0,65437″
Arcturus	-	-	1,28784
Capella	-	-	0,10887
Lyra	-	-	0,11281
Aldebaran		-	0,01104
Procyon	-	-	0,04998
		Sum	2,22491″

The result of this investigation is, that by admitting a motion of the sun towards λ Herculis, the annual proper motions of our six stars, of which the sum is 5,3537″, may be reduced to real motions of no more than 2,2249″.

When first I proposed λ Herculis as an apex for the solar motion, it may be remembered that a reference to future observations was made for obtaining greater accuracy *. Such observations we have now before us, in the valuable tables from which I have taken the proper motions of the six stars; and I shall prove that, with their assistance, we may fix on a solar motion that will be considerably more favourable.

We have already shown, that to ascertain the precise place of the best apex is attended with some difficulty; but from the inspection of the figure which represents the quantities of real motion required when λ Herculis is fixed upon, it will be seen that, by a regular method of approximation, we may turn the line SP into a situation where all the angles of the apparent motion of the six stars will be much reduced. The quantities which are required for constructing another figure to represent the threefold motions of our six stars, when a different apex is fixed upon, are to be found by the same method we have pursued in the instance of λ Herculis; and the figure that has been given with respect to that star, shows evidently that the parallactic line SP should be turned

* See Phil. Trans. for 1783, p. 273, line 8; and p. 274, line 4.

more towards the line *sa*, representing the apparent motion of Sirius. We shall accordingly try a point near the following knee of Hercules, whose right ascension is 270° 15, and north polar distance 54° 45′.

The result of a calculation of the angles, and the least quantities of real motion of our six stars, according to this apex, is collected in the following table, and represented in fig. 3.

Table VI.

Stars.	Angles of the parallactic Motion with the Parallel.	Angles of the apparent with the parallactic Motion.	Least Quantities of the real Motion.
	° ′ ″	° ′ ″	″
Sirius -	68 51 5 S. preceding	0 1 25 S. preceding	0,0004561
Arcturus	29 30 32 S. preceding	25 59 10 S. following	0,9145072
Capella -	77 54 0 S. following	6 18 38 S. following	0,0509727
Lyra -	27 38 47 N. following	28 42 9 N. preceding	0,1557761
Aldebaran	66 20 17 S. following	10 9 21 S. preceding	0,0217607
Procyon	64 48 27 S. following	14 46 1 S. preceding	0,3159051
			Sum 1″,4593779

By this table it appears that the annual proper motion of our six stars may be reduced to 1,4594″, which is 0,7655″ less than the sum in the 5th table, where the apex was λ Herculis.

In the approximation to this point it appeared, that when the line of the parallactic motion of Sirius is made to coincide with its apparent motion, we may soon obtain a certain minimum of the other parallactic motions; but as Sirius is not the star which has the greatest proper motion, it occurred to me that another minimum, obtained from the line in which Arcturus appears to move, would be more accurate; for, on account of its great proper motion, we have reason to suppose it more affected than other stars by the parallax arising from the motion of the sun; and, with a view to this, I soon was led to a point not only in the line of the apparent motion of Arcturus, but equally favourable to Sirius and Procyon, the remaining two stars that have the greatest motions.

If the principle of determining the direction of the solar motion by the stars which have the greatest proper motion be admitted, the following apex must be extremely near the truth; for, an alteration of a few minutes in right ascension or polar distance either way, will immediately increase the required real motion of our stars. Its place is: right ascension 245° 52′ 30″, and north polar distance 40° 22′.

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The calculated motions of the same stars by this apex will be as in the following table, and are delineated in fig. 4.

Table VII.

Stars.	Angles of the parallactic Motion with the Parallel.	Angles of the apparent with the parallactic Motion.	Least Quantities of the real Motion.
	° ′ ″	° ′ ″	″
Sirius -	58 24 56 S. preceding	10 24 44 following	0,20157
Arcturus	55 29 45 S. preceding	0 0 3 preceding	0,00003
Capella -	83 44 17 S. preceding	24 40 21 following	0,19358
Lyra -	36 28 33 S. following	92 49 30 following	0,32396
Aldebaran	89 48 35 S. preceding	13 18 58 following	0,02842
Procyon	59 43 10 S. preceding	9 40 46 preceding	0,20839
		Sum .	0″,95595

The sum of the real motions required, with the apex of the solar motion above mentioned, is less in this table than that in the former by 0,50343″.

In these calculations we have proceeded upon the principle of obtaining the least possible quantity of real motion, by way of coming at the most favourable situation of a solar apex; and have proved that the sum of the observed proper motions of the six principal stars, amounting to 5,3534″, may be the result of a composition of two other motions; and that the real motions of these stars, if they could be reduced to their smallest possible quantities, would not exceed 0,9559″.

But as I do not intend to assert that these real motions can be actually brought down to the low quantities that have been mentioned, it will be necessary to show that the validity of the arguments for establishing the method I have pursued will not be affected by that circumstance. In the first place, then, we should consider, that although the great proper motions of Arcturus, Procyon, and Sirius, are strong indications of their being affected by parallax, it does not follow, nor is it probable, that the apparent changes of the situation of these stars should be entirely owing to solar motion; on the contrary, we may reasonably expect that their own real motions will have a great share in them. Next to this, it is evident that in the case of parallactic motions the distance of a star from the sun is of material consequence; and as this cannot be assumed at pleasure, we are consequently not at liberty to make the parallactic motion sp in fig. 1, equal to the line sm of the same figure: hence it follows, that the real motion of the star cannot be from

from m to a, as the foregoing calculations have supposed; but will be from p to a. It is, however, very evident, that if ma be a minimum, the line pa, when sp is given, will also be a minimum; and if all the ma's in fig. 4. are minima, it follows also that all the sp's, whatever they may be, will give the pa's as small as possible: and this is the point that was to be established.

Whatever, therefore, may be the sum of real motions required to account for the phænomena of proper motions, our foregoing arguments cannot be affected by the result; for, as by observation it is known that proper motions do exist, and since no solar motion can resolve them entirely into parallactic ones, we ought to give the preference to that direction of the motion of the sun which will take away more real motion than any other, and this, as we have shown, will be done when the right ascension of the apex is 245° 52′ 30″, and its north polar distance 40° 22′.

XLVI. *Life of* John Bevis, *M. D. F. R. S. &c. Communicated by Mr.* T. S. Evans, *of the Royal Military Academy, Woolwich.*

John Bevis, M.D., fellow of the Royal Society of London, and corresponding member of the Academy of Sciences of Paris and Berlin, was born the last day of October 1695, (old style,) near old Sarum, in Wiltshire. His father distinguished himself very honourably, in the time of the Revolution, by raising and supporting, at his own expense, a company of infantry to assist king William, and expended in it the sum of 2000l., no part of which was ever reimbursed him by government.

The son, after receiving that kind of education which was necessary to qualify him for the university, was entered at Christ's college, Oxford, where he applied with great ardour, not only to the study of physic, for which he was intended, but also to many other sciences: he had, in particular, a strong partiality for optics, and was rarely without sir Isaac Newton's treatise on that subject in his pocket. This will not surprise us much, when we consider how necessary a knowledge of this science is to the one for the advancement of which he had so remarkable a predilection. Not contented with acquiring the theory of astronomy, he made himself also a proficient in the practical part, even while he was at college; and frequently amused himself

with polishing glass for optical purposes, in which he was quite an adept.

Having taken his degrees, as far as doctor of physic, he left the university, and made the tour of France and Italy, and on his return commenced the profession of medicine in London, where he had great practice. But the study of physic afforded him no pleasure to compare with that which he received from contemplating the divine works of the Omnipotent in the heavens.

As early as the year 1738 he had procured an excellent collection of astronomical instruments*, for the purpose of furnishing a new observatory, which he had built under his own particular direction, at Stoke Newington, on the north side of London. In this place, when he had established himself, he became a most indefatigable observer, which is proved by three volumes in folio, filled with observations, which he made between the 6th of March in that year and the 6th of March in the following year. From these he selected the most important, making one volume of 196 pages, on large paper, where it frequently occurs that the transits of one hundred and sixty stars, &c. have been observed in one night.

Dr. Bevis continued to observe the heavens with the same assiduity till the year 1745, when, finding he had collected sufficient materials, he imposed on himself the laborious task of arranging, and publishing by subscription, a work entitled "*Uranographia Britannica*; or An exact View of the Heavens, on fifty-two Plates;" similar to that of Boyer, representing the constellations and all the fixed stars that had been observed by astronomers, together with a considerable number that had only been observed by himself. In this work, which he announced in 1748, are two hemispheres, which represent the constellations as they were laid down by the antient astronomers†. To each plate he wrote a particular explanation, with remarks; and joined a complete catalogue of all the fixed stars, with their magnitudes and positions for that time.

These plates, which would have done honour to his country as well as to himself, notwithstanding they have been engraved for so many years, have never come before the public, on the following account:—He had engaged a person

* Among others he had in his possession a curious wire micrometer capable of an inclinatory motion, which once belonged to the celebrated astronomer Hevelius.

† Similar, perhaps, to plates 33 and 34 of Bode's small atlas.

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to engrave them whose name was John Neale *, and who, after he had received several hundred pounds of the subscription, became a bankrupt: by this means the work fell into the hands of Neale's creditors, and was at length put under the protection of the court of chancery, so that the author could never afterwards enjoy the fruit of great labour †. What was still worse, the subscribers to this excellent work, having been thus disappointed, supposed that Dr. Bevis had some connection with Neale in his misconduct; which being told him, he felt himself so much chagrined by it, that he never spoke of this unpleasant business during the remainder of his life, without feeling himself in some degree affected. Mr. Horsefall, whom he left his executor, and who was very fond of astronomy, used every means in his power to forward the suit after the doctor's decease: he even offered to give up his own interest in the affair, to bring it to a final conclusion, that the work might be published; but to no purpose. Very great pains had been taken with it; and, besides the previous labour, many whole days were spent by the side of the engraver, to see that every star was laid down in its proper position. At the commencement of the suit there was very little to finish for the press, all the plates being ready.

Dr. Bevis was the author of a great many works, which have been well received by the public; but his modesty would not permit him to take the merit of them to himself ‡. It is to him we are indebted for the publication of the celebrated Dr. Halley's astronomical tables, with whom he was intimately acquainted. They were left from the year 1725 till his death in the hands of the printer, where, perhaps, they might have shared the same fate as the atlas before mentioned, had not Dr. Bevis taken them up, and by supplying the necessary auxiliary tables, and precepts to use them, brought them to light in the year 1749 §.

In Mr. Thomas Simpson's Essays, p. 10, are given prac-

* This John Neale published a pamphlet containing some improvements in the barometer.

† Lalande says that the doctor showed him the proofs when he was in England in the year 1763, at which time they were still in custody, but that M. Messier had a set of them. (*Astr.* vol. i. p. 242, 3d edit.)

‡ Heath says, p. 234 of his Royal Astronomer, that Gael Morris was the precept writer to Dr. Halley's tables. Possibly G. Morris might assist Dr. Bevis, or actually write them under his inspection; but there is no doubt that Dr. Bevis was the responsible person concerned.

§ Dr. Halley expected to bring the theory of the moon to a greater state of perfection, and the publication of the tables was delayed for that purpose. M. de l'Isle, with whom Dr. Halley as well as Dr. Bevis corresponded, has published two curious letters on the history and theory of these tables.

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tical rules for finding the aberration, which were drawn up and given him by Dr. Bevis, together with examples of the corrections applied to several stars, which he had himself carefully observed with proper instruments; whereby, as Mr. Simpson observes, he has proved, the first of any one, that the phænomena are universally as conformable in right ascension as Dr. Bradley, who made this great discovery, found them to be in declination.

At a meeting of the Board of Longitude on the 18th September 1764, he was nominated, jointly with Mr. George Witchell and captain Campbell, to compute the observations made at Greenwich, and compare them with those made at Portsmouth and elsewhere, for the purpose of ascertaining the accuracy of Mr. Harrison's timekeepers.

To prove the estimation in which he was held by mathematical men, we need only observe, that the ingenious Mr. Crakelt has dedicated to him his translation of M. Mauduit's highly valuable *Astronomie Sphérique*; and the booksellers, on account of his well known literary erudition, requested he would write a letter of recommendation to a very useful little dictionary, which has always, on that account, gone by the name of Dr. Bevis's dictionary.

He has enriched the Philosophical Transactions, from vol. xl. to vol. lix. inclusive, with twenty-seven valuable papers, mostly containing astronomical observations; and he has inserted several things in the Mathematical Magazine, by Messrs. Moss and Witchell, particularly a curious paper on the satellite of Venus.

He announced, in the *Journal des Sçavans* for August 1771, an English translation of Lalande's Astronomy, done principally by himself; but it has never been published, although left ready for the press at his death.

The only things which have appeared separately with his name were two pamphlets; one entitled "The Satellite Sliding Rule," for determining the immersions and emersions of the four satellites of Jupiter; the other was "An experimental Inquiry concerning the Contents, Qualities, and medicinal Virtues of the two mineral Waters lately discovered at Bagnigge Wells, near London; with Directions for drinking them, and some Account of their Success in obstinate Cases:" 8vo. London, 1760. A second edition, with additions, was given by him in 1767.

About this time the ingenious Mr. John Dollond, of St. Paul's Churchyard, invented the method of correcting the aberration and colour of rays of light passing through a single object-glass, and thereby of shortening the length

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of telescopes, by using a compound object glass, composed of a convex lens of crown glass and another concave one of white flint glass; or else, by means of two convex ones of crown glass, and one concave one between them of white flint glass. Dr. Bevis was the first who gave the name of *achromatic* to telescopes made in this manner, which name has ever since been universally adopted both at home and abroad. This invention induced him to make some curious experiments on the refractive power of glass, in the composition of which he had used a quantity of borax, and found the refrangibility was about as great as that of English crystal.

The French astronomers had always received the credit of being the first inventors of the wire micrometer, until Dr. Bevis, in looking over some letters, the originals of which were in lord Macclesfield's library, found, by accident, that Gascoigne first invented it in the year 1641, whereas Auzout's letter to Mr. Oldenburg, which only mentions his having *used* it to measure the sun's diameter, was not dated till the 28th of December 1666.

M. Grischou, when he was at Leyden in 1749, engaged M. Schultens, professor of Arabic in that city, to translate the manuscript, in the public library of that city, containing the observations of Ibn Iunis, made at Jaffa, about six or seven miles from Cairo, in Egypt, in the years 977, 978, and 979, where are recorded, among other things, two remarkable eclipses of the sun. Dr. Bevis procured a copy of this manuscript in order to compare modern observations with these antient ones, for the purpose of settling the maximum of some equations in the solar tables; but in the course of his researches he found them so obscure and unsatisfactory that he was obliged to reject them. He afterwards presented this manuscript to M. de l'Isle, and it has lately been translated into French by M. Caussin, and inserted in the *Mem. de l'Instit. Nat.* tom. ii.*

At the death of Mr. Bliss, in 1765, his friends made great exertions to procure for him the situation of astronomer royal; but Dr. Maskelyne obtained it through the interest of the earl of Macclesfield, who was at that time president of the Royal Society. His majesty, notwithstanding he could not comply with his wishes in this instance, was very partial to him, and requested his assist-

* An analysis of this curious and valuable manuscript may be seen in the first volume of Dr. Garnett's Annals of Philosophy, p. 105. La Place has derived great benefit from the use of it.

ance

ance and advice in directing the building of the observatory at Richmond in 1770.

He corresponded with most of the principal astronomers in all parts of the continent; and many of them make very honourable mention in their works of the civilities and attention they received from him, either during their stay in England, or by communications to them abroad.

His diploma of member of the Royal Academy of Sciences of Berlin is dated the 11th of June 1750, and was accompanied by a very polite and flattering letter from the celebrated M. de Maupertuis, president of that academy, in which, speaking to him of the work above mentioned, he called it "your inimitable atlas."

The 12th of July 1768 he was chosen corresponding member of the Royal Academy of Sciences at Paris.

A few years before his death he left the house in which his observatory was at Stoke Newington, and removed into the Temple; but the improper situation of his house for astronomical purposes, the approaches of old age, his occupations as a physician, and at the Royal Society, now prevented him from continuing his observations so regularly as before. Nevertheless he had his astronomical clocks, quadrants, and telescopes, about him, to amuse himself occasionally, or to gratify a friend with the sight of any particular observation when it occurred in the heavens.

His death was occasioned by a fall which he received a short time before, in going rather too hastily from his instrument to the clock, in observing the meridian altitude of the sun. He died the 6th of November 1771, aged 76, in the Middle Temple, perfectly free and resigned, and with that constancy and serenity of mind becoming a christian and a philosopher.

Dr. Bevis's disposition was lively, amiable, and liberal: he could never see any one in embarrassment, of whatever country or religion he was, without sympathizing in his distress, and affording him relief if possible.

He rendered very essential service to astronomy by the great encouragement and assistance he gave to astronomers, as well of his own country as foreigners. He was always ready to see them, and never refused his advice when they either wanted to purchase instruments or make observations; and, in general, never spared any pains or trouble that contributed to forward the progress of astronomy.

XLVII. *On*

XLVII. *On the magnetic Attraction of Oxides of Iron.* *By* Timothy Lane, *Esq. F.R.S.**

Having found, by experiment, that hardened iron is not so readily attracted by the magnet as soft iron, and that needles are inferior to iron wire as indexes to Six's thermometer, I was proceeding to other comparative experiments when I received the Second Part of last year's Philosophical Transactions, in which I saw an Analysis of Magnetical Pyrites, with Remarks on Sulphurets of Iron, by Mr. Hatchett.

This paper led me to examine what magnetical properties iron possessed when free from inflammable matter. For this purpose I obtained a precipitate of iron prepared and sold at Apothecaries' Hall by the name of *ferrum præcipitatum*. Mr. Moore, the chemical operator, informed me that he prepared it by dissolving twelve pounds of sulphate of iron in twenty-four gallons of distilled water, and then adding eight ounces of sulphuric acid to render the solution more complete. Twelve pounds of purified kali were mixed with the solution: the precipitate was well washed with hot distilled water, and then carefully dried. This precipitate is similar to the sediment of chalybeate waters, and affords no magnetic particles; nor, when exposed to a continued clear red heat, does it suffer any alteration beyond the acquirement of a darker colour. But if any smoke or flame has access to it, then magnetic particles are evident. Heat, by the converging rays of the sun †, equal to that at which glass melts, blackens the oxide, but does not render it magnetic, if free from any inflammable matter. It is requisite, in this experiment, to protect the oxide, by glass, from the dust floating in the air, which otherwise will render many of the particles magnetic. I attributed this effect to the deoxidizing property of light, till, by employing a protecting glass, the result proved it to proceed from the dust in the atmosphere.

By repeated experiments I found that heat alone produced no magnetic effect on the oxide, and that inflammable matter with heat always rendered some of the particles magnetic.

As the inflammable matter in coal had this effect, I mixed some of the oxide with a portion of coal in a glass mortar, and continued rubbing them together for some time without

* From the *Transactions of the Royal Society* for 1805.

† The lens employed in this experiment was twelve inches in diameter, and the heat at its focus was sufficient to melt iron: from Mr. Dollond.

any

any magnetic effect. The mixture was then put into a tobacco-pipe, and placed in the clear red heat of a common fire: as soon as the pipe had acquired a red heat, it was taken out. The mixture was put on a glazed tile to cool, and proved highly magnetic.

I rubbed a portion of the original oxide in a glass mortar with a variety of substances, as sulphur, charcoal, camphor, æther, alcohol, &c., and found that no effect was produced without the assistance of heat. The heat of boiling water, moreover, was not sufficient; but by the heat of melting lead I procured magnetism. Small quantities of any inflammable matter in a red heat have an evident effect on the oxide. Hydrogen, aided by a red heat, renders the oxide magnetic. Alcohol has the same effect. But if the alcohol be diluted with water, though it may flame in the fire, it will be ineffectual, as it is driven off before the oxide becomes sufficiently heated to receive its action.

Such combustible substances as do not very readily part with their carbonic element, require rather longer continuance of heat than others; for example, charcoal and cinders, well burnt, must be longer in the fire to have their full effect on the oxide, than dry wood, coal, or sulphur.

But such substances as may be sublimed with facility, will gradually quit the oxide, by a continued application even of a low heat, leaving it unmagnetic, as at first.

How very small a portion of inflammable matter is requisite to render a considerable quantity of oxide magnetic is evident, since one grain of camphor dissolved in an adequate portion of alcohol, and mixed with a hundred grains of the oxide in a glass mortar, will, by a red heat, render all the particles of the oxide magnetic.

As oxides of iron, therefore, are rendered magnetic by heat, when mixed with inflammable matter, it may be understood why Prussian blue, sulphurets, and ores of iron containing inflammable matter, become magnetic by the agency of fire; while at the same time it is observable that these same ores revert to their unmagnetic state, when the heat has been continued sufficiently long to drive off the whole of the inflammable matter: thus we find among the cinders of a common fire calcined sulphurets of iron, distinguishable by their red colour, unmagnetic when all the sulphur is sublimed.

My intention in this communication is to prove generally that mere oxides of iron are not magnetic; that any inflammable substances mixed with them do not render them magnetic until they are by heat chemically combined with

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the oxides, and that when the combustible substance is again separated by heat, the oxides return to their unmagnetic state. That magnetic oxides cannot be distinguished from calcined oxides by their colour. I entertain a hope, however, that this subject may be found worthy of the accurate investigation of some other member of this learned society.

XLVIII. *Extract from a Memoir of Messrs.* Fourcroy *and* Vauquelin *upon the Discovery of a new inflammable and detonating Substance formed by the Action of Nitric Acid on Indigo and Animal Matters. By* A. Laugier *.

The application of the nitric acid to vegetable and animal matters has produced, it is well known, a multitude of important discoveries. The disengagement of a part of the azot of animal compounds, and their conversion into oxalic acid, as observed by M. Berthollet, together with the discovery of the formation of ammonia and the prussic acid by M. Fourcroy, form a brilliant æra in the history of chemical science. The changes which organic compounds suffer by the action of nitric acid, which produces nine or ten different substances, themselves compounds, are so multiplied and various, that they excite the astonishment of chemists, and induce them to regard this action of the nitric acid as a rich mine to labour in: that it is still far from exhausted will appear from the discovery of two substances, hitherto almost wholly unknown, which form the subject of this memoir.

The most remarkable of these is produced by boiling nitric acid upon animal substances or vegetables containing azote. It is of a yellow colour, has an intensely bitter taste, and is distinguished by its property of inflaming and detonating with violence when exposed to a moderate heat.

M. Haussmann, by a memoir which appeared in the *Journal de Physique* (March 1788), where he relates some experiments on indigo with the acids, seems to have seen this substance. Although he confounds it with the oxalic acid, yet its properties are pointed out by him with sufficient accuracy; its bitterness, its yellow colour, its solubility, and its precipitation by alkalies: but its principal property

* From the *Annales de Chimie*, No. 165.

of

of inflammation and detonation, of course its intimate and peculiar nature, has altogether escaped him.

The substance termed by Welther the bitter principle, in which he discovered a power of detonating, seems to be the same matter: but he has attributed this peculiarity to the presence of a portion of nitrate of potash.

The most convenient mode of procuring the substance in question, is to boil four parts of nitric acid, of 18 or 20 degrees strength, upon one part of powdered indigo of Guatimala, until its colour is destroyed, while the acid becomes yellow, and till there remains on the surface only a thin layer of resinous matter, which becomes firm by cooling. This is to be removed, the solution evaporated to the consistence of honey, and the residuum dissolved in hot water, and filtered. A solution of the potash of commerce is now to be poured into the liquor, when a number of small yellow crystals of a circular shape will appear, forming the inflammable substance.

The resin which has been separated may, by the addition of a new portion of acid, be also converted into the same yellow detonating substance.

If the process be stopped before the point mentioned, we obtain, instead of the detonating substance, a matter of a yellow colour and crystallized, but more soluble in water, and subliming in the form of white needles.

This substance exhibits all the characters of benzoic acid, altered by a portion of the resin. In all probability the continuance of the process decomposes or volatilizes this acid.

The orange colour of the detonating matter; its bitter taste; its solubility in boiling water, in alcohol, and, above all, in nitric acid; the very deep blood-red colour which it acquires on the application of alkalis, and which it communicates to the precipitate from the sulphate of iron; the tenacity with which it adheres to the benzoic acid which is formed along with it by the action of the nitric acid upon indigo; and lastly, its property of detonating strongly with a clear purple light when wrapped up in a bit of paper and struck with a hammer, are characters which sufficiently distinguish this substance from every other with which we are acquainted.

The celebrated authors of this memoir have ascertained that the detonating property of the new substance depends neither on the presence of nitric acid nor on that of ammonia; for concentrated sulphuric acid disengaged from it no acid

acid vapours, and caustic potash no ammonia. They are, on the contrary, inclined to believe that potash has some share in the effect of detonation; since acids in which this substance has been digested contain traces of salts having potash as their base. When deprived of alkali this substance is more soluble in water, and crystallizes in elongated plates of a yellow colour and bitter taste, having acid characters: these crystals, if moistened with potash, resume their detonating property. The potash seems merely to render this substance more fixed; to favour the accumulation of caloric, and to determine, consequently, the combustion of the elements which compose it; viz. of the carbon, the hydrogen, and perhaps of the azote, by means of the oxygen which it also contains.

Indigo is not the only substance which furnishes this detonating composition; the muscular fibre treated with nitric acid presents the same phænomena; and it is probable that silk, wool, and other animal and vegetable matters containing azote will also furnish it.

The labours of Messrs. Fourcroy and Vauquelin present two very interesting facts. It follows, 1. That the benzoic acid may be formed from a multitude of different substances, which we were formerly ignorant of: 2. That animal and vegetable substances containing azote, if treated with the nitric acid, which takes from them a portion of carbon, of hydrogen, and of azote, give birth to a matter supersaturated with oxygen, and possessing the property of detonation. This substance, which the authors of the memoir have examined with care, appears to them to be a superoxygenated hydro-carburet of azote.

XLIX. *Third Communication from Mr.* W. Peel, *of Cambridge. On the Production of Muriates by the Galvanic and Electric Decomposition of Water.*

To Mr. Tilloch.

SIR,

I feel no little satisfaction at the interest my experiments have excited, and the favourable reception they have met with from you.

I have now the pleasure to inform you, that since my last letter, dated the 4th of June, (Phil. Mag. vol. xxii. p. 153.) having had some leisure time upon my hands, I have dedicated as much of it as my health would permit to the

the further investigation of the subject on which I then wrote you.

In my first letter, (vol. xxi. p. 279.) dated the 23d of April last, I informed you, briefly, that by decomposing, by means of Galvanism, about one-half of a whole pint which I employed of distilled water, I had obtained in the remaining water a quantity of *muriate of soda*. In my letter of the 4th of June I informed you, that on repeating the same experiment with water formed by the combustion of hydrogen with oxygen gas, *saturated with lime* to free it from some free nitric acid with which it was found tinctured, and then distilled, I had, instead of muriate of soda, as in the former experiment, obtained muriate of potash.

The experiments which I am now to state, were undertaken,

1st, To determine whether the difference in the result of the before-mentioned experiments was owing in any degree to my having employed lime to neutralize the water employed in my second experiment, before it was distilled.

2d, To ascertain whether the salts found in the residual water, or any component part of them, came from the Galvanic battery by means of the wires.

To determine the first point, I varied my experiment by employing for decomposition water distilled under different circumstances.

Exp. I. The water employed in this experiment was distilled from water containing *lime*. A portion of it was decomposed in the manner that has before been stated. The remaining water yielded *muriate of potash*.

Exp. II. Water distilled from water containing *magnesia* was decomposed in the same manner. The result was *muriate of soda*.

Exp. III. In this experiment double distilled *snow water* was employed. The result was *muriate of soda*.

Exp. IV. Water distilled from *barytes* was now used. The result was still *muriate of soda*.

The water which I used in the experiment detailed in my first letter was distilled from pump water (the pump is on the premises where I live), which I have not myself analysed, but a friend has been so good as to take upon him that trouble. He has not been able to detect in it the minutest portion of magnesia. In one of the above experiments, having used water distilled from magnesia, I obtained muriate of soda; but, having obtained the same result from distilled snow water, and from water distilled from barytes,

barytes, I conclude that the production of the soda has nothing to do with the presence of magnesia.

But in the production of potash the presence of lime seems to be essential, and, as you hinted, a portion of lime must have been carried over with the distilled water; a fact which few would suspect, and which probably may often be the cause of differences in the results of chemical investigations, conducted, to all appearance, in a similar manner.

To determine the second point which I had in view; namely, whether the salts found in the residual water, or any component part of them, came from the Galvanic battery by means of the conducting wires, I made similar experiments to those before stated, employing for the decomposition of the distilled water a powerful electrical machine instead of a Galvanic battery, but without obtaining results different from what have been already stated.

I am now engaged in an experiment concerning the formation of potash; but, being desirous that my present letter, which I write in haste, may reach you in time for this month's Magazine, cannot wait for its result. Should the experiment succeed, you shall be informed as soon as possible. In the mean time I remain, with the greatest regard, yours,

W. Peel.

Cambridge,
Dec. 20, 1805.

L. *Experiments on Gum Arabic and Gum Adraganth.* *By M.* Vauquelin*.

1. Ten grains of red gum adraganth, furnished by combustion three decigrammes and a half of white ashes. These ashes dissolved in muriatic acid with effervescence, and gave out an odour of sulphurated hydrogen. Their solution gives by ammonia a precipitate which consists of phosphate of lime and oxide of iron. The oxalate of ammonia precipitates a considerable quantity of lime.

Thus the red gum adraganth contains in a hundred parts about three and a half of ashes, which are composed, for the greater part, of carbonate of lime, of a small quantity of iron, of phosphate of lime, and perhaps also a minute portion of alkali.

2. Ten grains of white gum adraganth, submitted to the same proofs, yielded three decigrammes of ashes, which

* From the *Annales de Chimie*, tom. liv.

were composed of the same principles as those of the red gum. Further, in washing there was extracted a small quantity of alkali, viz. potash.

3. Ten grains of gum arabic, burnt like the others, left three decigrammes of ashes, which were composed of the same elements with the preceding; with this difference, however, that they gave no symptoms of the presence of an alkali, nor of sulphur, as the others.

I had formerly suspected that the opacity of gum adraganth, and the difficulty which it has to dissolve in water, were owing to the existence of a very great quantity of earthy matter; but after these experiments it appeared they were owing to another cause.

To a certainty the lime is not found in the state of a carbonate, and still less in the state of quicklime; for the solutions of gum are not alkaline, they are, on the contrary, slightly acid. At least, if one rub on a morsel of gum a piece of test paper well moistened, it is sensibly reddened. It is certain, also, that oxalate of ammonia and carbonate of potash occasion precipitations in the solution of gum arabic, and that the acetate of lead is not there formed at all. It follows from thence that there is lime in the gums, combined with an acid:—But what is this acid?

Here, for want of facts, I shall be obliged to give way to conjecture; but conjecture very probable, which every thing seems to support, and nothing to contradict. It is not doubtful, at least, that it is a vegetable acid; for, on being decomposed, they leave their bases combined with carbonic acid.

Supposing this, let us see among the great number of acids that which could best satisfy all those conditions. It is neither the oxalic, nor the tartareous acid, nor the citric, because their combinations with lime are insoluble in water, and likewise because they exist in only a small number of vegetables. Still less can it be the benzoic, the gallic, the *mosoxalic* or the *honistic* acid; which, as we know, are very rare in nature, and of which the three first form also salts which are very little soluble.

It remains, therefore, only to choose between the acetous and the malic acid. The first forms, as we know, soluble combinations with all known substances with which it is capable of uniting; some of them are even deliquescent. It is, besides, a result most frequent in the operations of nature in the vegetable and animal system; since it forms itself by vegetation, fermentation, the action of powerful acids, and the influence of heat.

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The combinations of the second are the greater part insoluble in water; that which it forms particularly with lime is not sensibly soluble but by the aid of an excess of acid, and its existence in nature is not so frequent as that of the acetic acid; and since the lime which is found in transparent gums has been incontestibly dissolved in the juices of the vegetables which furnished these substances, it is much more probable that this earth is there combined with the acetic acid than with any other.

It is very probable that the small quantity of potash which I found in the ashes of the burnt gums is united to the same acid, which would explain why these substances are so sensible to humidity, and soften themselves so as not to be pulverizable.

I am, however, very much disposed to believe, that in certain opake adraganth gums difficult to dissolve, and which give much lime by incineration, this earth is there combined with the malic acid. I have had occasion to examine lately a gum collected by M. Pallissot-Bauvois on the nopal of cochineal, which was opake, swelled itself in water, but did not dissolve in a homogeneous manner, and which gave eight per cent of lime.

As the sap of all the cactus which I have submitted to analysis has afforded me quantities more or less considerable of the acidulous malate of lime, one may presume, with sufficient reason, that the kind which nourishes the cochineal contains it also; and that it is the presence of this salt, proceeding from the vegetable when dissolved in the sap with the gum, which gives it its opacity, and hinders its solution in water.

It results at least from these experiments, that the gums contain, 1. A calcareous salt, most frequently the acetate of lime: 2. Sometimes a malate of lime with excess of acid: 3. Phosphate of lime: 4. Iron, which is probably united to phosphoric acid.

LI. *A proposal for destroying the Fire- and Choak-Damps of Coal Mines; and their Production explained on the Principles of modern Chemistry: addressed to the Owners and Agents of Coal Works, &c.*

The subjoined remarks are extracted from a little tract which professes only to hold out a short explanation of well known facts, in the hope of seeing them conducive to save human beings whose labours are useful to the community.

munity. It is written in a popular manner, and, we think, deserves the attention of those to whom it is addressed.

The decomposition of water when *carbonaceous* matter, or *coal*, comes in contact with it, and the affinity which the oxygen of the water has for carbon, account satisfactorily for the formation both of *fire-damp* (hydrogen gas) and of *choak-damp* (carbonic acid gas). The hydrogenous gas, from its specific gravity being so much less than that of atmospheric air, as 13 to 1, or 16 to 1, if perfectly pure, ascends to the roof of the mine: the carbonic acid gas, owing to its specific gravity being more than twice that of common air, falls to the bottom. When these two do not by their bulk fill the space between the floor and top of the mine, a stratum of atmospheric air lies between them, being neither so heavy as the *choak-damp* nor so light as the *fire-damp*. Neither the hydrogen nor carbonic acid gas are respirable, if unmixed with atmospheric air; and wherever the former takes fire it must be in contact or mixture with atmospheric air, as it cannot be inflamed without it:—the result of the combustion is the formation of water which consists of hydrogen 0·15 and of oxygen 0·85; and thus pit-men who have been scorched with the explosion of fire-damp appear as if drenched with water.—Such, briefly, are the facts brought to view by the author, Dr. Trotter.

Dr. Trotter, in prosecuting his object, points out, 1st, The means to prevent the formation of noxious airs in the mines: 2d, For removing them when formed.

For the first, the mines should be as well ventilated as possible by the usual means: but one great object ought to be to prevent all stagnation of water; for where there is no moisture there can be no generation of the foul airs. Where-ever water can stagnate, a stream of pure water should be admitted at intervals to dilute and sweeten it, that the whole may be pumped out; and no chips of wood, or horse dung, should be allowed to mix with the water that may become stagnate.

To destroy fire-damp "we have only to employ some of the stronger acids in a state of vapour, such as the acetic, nitrous, or oxygenated muriatic. These acid vapours seize the hydrogen. In the expansible state of the acid gas its oxygen quits the radical or base of the acid and attracts the hydrogen; water is recomposed, but the caloric disengaged during the combination of the oxygen and hydrogen converts it into steam, so that it is not seen in a condensed state. This is the whole secret of destroying hydrogenous gas, or fire-damp." The oxygenated muriatic acid gas, on account

count of the extra oxygen which it contains, is the best for this purpose.

"The utensils required for this business are small flat stone dishes, made thick, about two inches deep; and a glass funnel for pouring in the acid of vitriol. The ingredients are, common salt (it ought to be bay salt), oxide of manganese, and concentrated acid of vitriol, which, in common language, is the strongest oil of vitriol.

Proportion for one Fumigation.

	oz.	dr.	grs.
"Take of common bay salt -	3	2	10
Fine powder of black manganese	0	5	17
Water - - - - -	1	2	33
Strong sulphuric acid - -	1	7	50

"After pounding the salt and manganese together, they may be put into the stone ware dish, and the water poured upon them; and afterwards the sulphuric acid, slowly, through a glass funnel. This quantity is sufficient for a space of 16 feet by 12; but the frequent employment must depend on the manner how the *fire-damp* is evolved."

To remove the choak-damp from coal mines the author recommends the use of water.

"Water, about the temperature of 40° of Fahrenheit, is found to dissolve equal parts of its bulk; but as the water of a deep pit is commonly above 50°, it will take up two-thirds of its bulk only. In order to effect this mixture with water, I would recommend (says the author) the common fire-engine, such as is used in the case of fire. The workmen might for safety stand at a distance, and by directing the mouth of the tube to the spot where the *choak damp* is known to lie, the water may be so diffused as to take up the whole. The water will then taste acidulous, and lights will burn, and animals breathe, in the place whence the vapour was dislodged. That the diffusion of the water might be more speedy in dissolving the *choak-damp*, the tube might be fitted after the manner of a garden watering-pot, so as to sprinkle and break the fluid into a shower.

"This kind of air being speedily attracted by quicklime, by mixing that article in the water which is to be diffused would still more effectually dislodge the *choak-damp*; and in places where it happens to be collected in great quantity, such a mixture would be highly serviceable.

"It ought to be remembered, whenever either *fire-damp* or *choak-damp* are detected in coal-pits, that there will be reason to fear a collection of the other near the spot, if not

 powerfully

powerfully ventilated; for it is without doubt that they are invariably generated by the same process and at the same time."

Such are the means proposed by Dr. Trotter for destroying the *fire-* and *choak-damps* in coal mines. The ingredients, he observes, are so cheap, that "the largest mine could not consume one hundred pounds (value, we suppose, he means,) in the year, by keeping the fumigation in constant use day and night." But the constant use of it would, we think, be impracticable; for too free an inhalation of oxy-muriatic acid gas is apt to produce pleurisy, and many people fell victims to this malady in different works when this gas was first introduced for the purpose of bleaching. The intention of the author is, however, not the less praiseworthy; nor should this oversight militate against a fair trial, judiciously conducted, of the means he has suggested. We believe few or none of the mines are worked in the night-time. Before quitting the mine the fumigating pans might be properly disposed, and the desired end probably be gained before the people return to their work in the morning.

LII. *Letter of M.* Gay-Lussac *to M.* Berthollet, *on the Presence of Fluoric Acid in Animal Substances**.

My honoured friend,

When I quitted Paris, about four months ago, you were at that time unacquainted with the existence of fluoric acid in animal substances; and I have since received from you no notice of that beautiful discovery. I conceive, therefore, that you will be gratified by the communication of what I have learnt respecting it from M. Morichini, a Roman chemist, by whom this discovery was made. As it was announced by him so early as the year 1802, you will no doubt be astonished that it should have remained so long unknown in France, and excited so little the attention of chemists.

In April 1802 the skeleton of an elephant was found in the neighbourhood of Rome, an account of which has been published by count Morozzo in the *Journal de Physique* of Paris. The *dentes molares* of this fossil elephant are formed of two very distinct substances; the inner of these is os-

* From the *Annales de Chimie*, No. 165.

seous,

seous, translucent, of a pale yellow somewhat resembling horn; the outer, or enamel, is very white, breaks easily, and has a rough fracture. The osseous portion is most observable on the upper surface of the tooth, where it forms triangular figures having rounded angles, and passes in plates even to the root of the tooth, filling up the spaces left by the enamel. On examining these two substances, count Morozzo conceived that they must be different, and begged M. Morichini to analyse them. This chemist soon discovered, by the application of the sulphuric acid, that the enamel of the *molares* of the fossil elephant was almost entirely composed of the fluate of lime, together with a small proportion of the phosphate and carbonate of the same substance: the osseous substance, however, he found to be principally formed of phosphate of lime. Such are the first general results obtained by M. Morichini: they have been published in the Memoirs of the Italian Society in this imperfect state, as he had not then leisure to extend his experiments further. Since that time, however, he has ascertained, with accuracy, the nature and proportions of the several substances composing the fossil tooth: he has also analysed the enamel of human teeth. M. Josse, as you well know, analysed this last; but there was still a great deal to be done; for his experiments offered no satisfactory account of the difference existing between the enamel and the bony part of the tooth. As M. Morichini has assured me his late experiments will certainly appear in the Memoirs of the Italian Society, I shall not enter into any detail of them, but merely relate the principal results.

M. Morichini, having separated a portion of the enamel of human teeth, and suspecting that it resembled in its composition that of the *molares* of the elephant, treated it in the same manner, and discovered, to his great satisfaction, that it contained a large proportion of fluate of lime. And to render his experiment more conclusive, he submitted to the same tests the two enamels and the fluate of lime. Under the action of the concentrated sulphuric acid, the last of these three substances gave out readily copious vapours of fluoric acid: the enamel of the fossil teeth emitted them rather more slowly, and that of human teeth with still less rapidity. M. Morichini, however, remarks, that this difference is wholly owing to the presence of an animal substance in the two enamels, which is more abundant in that of human teeth than in the enamel of the fossil; and to prove it, he asserts that we may retard the disengagement of the acid from fluate of lime by adding to it, after calcina-

 tion,

tion, a small quantity of gelatine, and then drying it. The vapours which these three substances yield by the sulphuric acid, exhibit equally the property of corroding glass, and depositing siliceous concretions in water, or on a moist sponge. They present also other characters which it would be useless here to mention, but which, taken in conjunction with the above, will not permit us to doubt their identity.

According to the experiments of M. Morichini, 100 parts of the enamel of human teeth contain thirty of animal substance, a little magnesia, alumine, and carbonic acid; and twenty-two of the fluoric and phosphoric acids, in combination with lime. He has been unable to separate accurately the fluoric from the phosphoric acid, but conceives that this last exists only in very small quantity.

The enamel of the teeth of the fossil elephant is likewise composed of the same elements, but in different proportions: thus it contains less phosphoric acid than that of the human teeth, and much less animal substance. M. Morichini, however, believes that this difference in the quantity of phosphoric acid in the two enamels very probably arises from a portion of the bony matter of human teeth, which it is very difficult to separate completely from the enamel. The most interesting result, and that which we least suspected, is the existence of fluoric acid in animal substances; a discovery of very great value, and one from which we may justly expect the most important consequences.

These conclusions respecting the presence of fluate of lime in the enamel of the teeth, are no doubt contrary to our former ideas upon this subject; for M. Josse, who has analysed the enamel of human teeth *, and Mr. Hatchett †, who examined that of a fossil elephant's tooth, have both discovered no other substance besides the phosphate of lime. The opinion of these two chemists induced M. Morichini to submit his own to rigorous examination: but his experiments have been so multiplied that there can now be entertained hardly any doubt upon this subject.

Pelletier has proved that the phosphate of lime of Estramadura contains a portion of fluate of lime, and that the earth of Mamarosch consists of a large proportion of this last, with a small quantity of phosphate. M. Morichini, reasoning from his own experiments, which prove that these two salts may be found united in animal substances, conceives it very possible that the phosphate of lime of Estra-

* *Annales de Chimie.* † Phil. Trans. 1799.

madura,

madura, and the earth of Mamarosch, may have arisen from the decomposition of the skeletons of large terrestrial animals.

Upon learning that the fluoric acid existed in the enamel of teeth, I immediately recollected that Rouelle had formerly announced that he was unable to procure phosphorus from ivory; and I suspected that it might be similar in its composition to enamel. I therefore calcined a quantity of ivory, and poured upon it concentrated sulphuric acid: vapours were now disengaged, which I readily recognised to be those of the fluoric acid. The tusks of the boar afforded the same results. When I say that these two substances contain fluate of lime, I would not be understood to assert that they are wholly composed of it, because I had no means of making an accurate analysis; the vapours of the fluoric acid, however, are so abundant, that we should be led to believe its combination with lime forms a very large part of their composition.

A few days after these experiments I examined, along with M. Morichini, a portion of fossil ivory which had been found in the neighbourhood of Rome. Its concentric layers, both external and internal, yielded the fluoric acid in great abundance. I attempted also to analyse the bones of fish; but I have made only a single experiment on those of the tench; and from this I confess I dare not say any thing decisive.

You perceive then, my friend, that it is highly probable the enamel of the teeth of all animals is formed, in a great measure, of the fluate of lime; and it may be remarked, that their canine teeth are either entirely composed of the enamel, or contain a much larger proportion of it than the others. As we have hitherto found in the urine all the different substances composing the bones, have we not ground for believing that it also contains the fluoric acid? If at a certain period of life the fluate of lime can be deposited upon the teeth, it appears necessary that when these have acquired their full growth this substance should find some outlet from the body.

What is the origin of this fluate of lime in animal substances? Although the presence of fluate of lime in the phosphate of lime of Estramadura has occasioned a suspicion that one of the acids forming these salts may be a modification of the other, this opinion appears to me to have but little foundation; for the co-existence of the two substances proves nothing as to their intimate nature: and we may readily convince ourselves, by simply inspecting a fossil

fossil elephant's tooth, that the osseous part is terminated by a distinct line, and even smooth on one side; which could not possibly be the case were the enamel changeable into osseous matter, or this last into enamel. If the fluoric acid is not formed in the bodies of animals, which seems the most probable opinion, but is only carried thither by means of the substances which furnish their nourishment, we ought then to find the acid in these substances.

Thus, my friend, you see what extensive and important inquiries still remain to be made upon the single point of the existence of fluoric acid in animal substances. M. Morichini, to whom this subject of right belongs, proposes to undertake the investigation: the field, however, is so wide, that it will furnish sufficient employment for the exertions of many chemists.

LIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY, LONDON.

The regular meeting of this society, which should have taken place on the 28th of November, was postponed to the 12th of December, the former having been appointed for a day of general thanksgiving.

On Saturday the 30th of November, being St. Andrew's day, the Royal Society held their anniversary meeting at their apartments in Somerset Place, when the gold medal (called sir Godfrey Copley's) was presented to Humphrey Davy, esq. for his various communications published in the Philosophical Transactions.

Afterwards the society proceeded to the choice of the council and officers for the ensuing year, when, on examining the ballots, it appeared that the following gentlemen were elected of the council:

Of the old council,—The right honourable sir Joseph Banks, bart. K. B.; sir Charles Blagdon, knt.; Henry Cavendish, esq.; Davies Giddy, esq.; Edward Whitaker Gray, M. D.; right honourable Charles Greville; William Marsden, esq.; Rev. Nevil Maskelyne, D. D.; George earl of Morton; Samuel Horsley, lord bishop of St. Asaph; William Hyde Wollaston, M. D.

Of the new council,—Mr. John Abernethy; George earl of Egremont; George Trenchard Goodenough, esq.; honourable Robert Fulk Greville; Philip Metcalf, esq.; Matthew Montague, esq.; lieutenant-colonel William Mudge; John

John Townley, esq.; William Charles Wells, M.D.; Thomas Young, M.D.

And the officers were,—The right honourable sir Joseph Banks, K.B. president; William Marsden, esq. treasurer; Edward Whitaker Gray, M.D., William Hyde Wollaston, M.D., secretaries.

Afterwards the members of the society dined together, as usual, at the Crown and Anchor Tavern, in the Strand.

On Thursday the 12th of December, the right honourable Charles Greville in the chair, minutes of the transactions of the council were read, with the above lists of officers, and a state of the funds of the society.

Letters were read from Drs. Maskelyne and Herschel mentioning their having observed a comet on the evening of the 8th instant, about six o'clock. It appeared like a star of the third or fourth magnitude, with a large coma, but no tail. It was not visible the succeeding evening.

A paper was also read on the dissection of a peculiarly formed heart, by Mr. Wyatt. It belonged to the body of a young woman who died of a mortification in her feet at the age of twenty. For forty days before her death, a chlorotic paleness pervaded her whole frame; her feet and legs were somewhat swelled, and painful; and her appetite continued good till the day she expired. When her body was opened by Mr. Wyatt, he found the heart nearly divided by a callous substance that obstructed its functions in receiving and discharging the blood through the aortas. To this cause, the obstructed or imperfect circulation, he ascribed the mortification of the extremities. The paper was illustrated by a drawing of the singular appearance of the heart.

Thursday, December 19, the right honourable Charles Greville in the chair, commenced the reading of a paper on guiacum, by Mr. Brandé, communicated by Mr. Hatchett, who has placed an accurate knowledge of this subject among the chemical desiderata, where, we fear, it must yet remain. Mr. Brandé agrees with Mr. Hatchett that guiacum is a peculiar substance, which he chooses, for the present, to denominate an extracto-resin, differing, as he says, from the other resins in having an extraordinary quantity of what is usually denominated extractive matter. Its peculiarities seem to consist principally in its different solutions in the acids being of very different colours, all of which are ascribed by Mr. Brandé to the presence of oxygen. Some, however, may be inclined to believe that different combinations of matter must present different surfaces or powers

powers of reflection; and that, consequently, a great variety of colours may be produced without having recourse to the magical operations of oxygen on every occasion. We would also observe, that some order should be adopted in detailing an account of numerous experiments on particular substances, such as balsams, gums, or resins. A multiplicity of isolated facts, in a confused mass, without any regard to the influence of some general law, can do little to facilitate the progress of knowledge; the memory cannot retain them, and we are reduced to the necessity of operating, as we have known some of the French chemists, with our book resting on the apparatus. It is true, these observations are no more applicable to the present author than to almost every other operator; but they must be evident to all who either read, write, or make experiments: and, as it should be a rule never to operate by a system, so it should also be a maxim, in reporting the results, always to arrange the facts according to some general analogy, drawn from just conceptions of the products of our operations. The experiments of Mr. Brandé, however, seem to have been well conceived, and are detailed with sufficient perspicuity and accuracy; but they are not, perhaps, so numerous as may be necessary to satisfy our curiosity respecting the peculiar principles of this useful medicinal drug.

SOCIETY OF ANTIQUARIES.

Nov. 28. An impression of a seal belonging to Mary queen of Scots, now in the museum at Paris, was exhibited. It contained the arms of England, against which Elizabeth remonstrated with so much ardour, denying Mary's right to retain the English arms, and alleging, from this circumstance, her ultimate views on the throne of England. The plea of Mary was, That her family connection gave her the right to use the arms of her forefathers, in which she was supported by the king of France. Several illustrative observations were made by the learned secretary who exhibited it, from which it did not appear that Mary's right in this case was sufficiently established by the usage of monarchs. This subject might again exercise the pen of the truly learned, highly ingenious, but too often fanciful, Mr. Whitaker.

Thursday, Dec. 12, the Rev. Dr. Hamilton in the chair, a polished stone was shown, supposed to be used by butchers before the use of knives, or the knowledge of iron. It was found in Suffolk, and consisted of shaded grayish green marble, well polished, very compact, except about an inch square

square near the broad end, that was very porous and rough: it is about 8 inches long, nearly 3½ inches broad at one end, and 1½ at the other, both of which are bevelled into a tolerably sharp and circular edge.

A very interesting communication from the director, Mr. Lysons, was read, on the state of the English mint, and coinage of England, during the reigns of the first six Edwards. From these curious items it appeared that this country was principally indebted, in the eleventh and twelfth centuries, to the merchants of Lucca for the current coin of the realm. Lucca was formerly a republican state of Italy, whose merchants then possessed the principal traffic of Europe.

J. B. Repton, esq. exhibited to the society some of his drawings of Saxon architecture, consisting of a capital and base of a Saxon column.

Dec. 19, the right honourable the earl of Leicester in the chair, a medal of Charles I., and a silver seal of the parish of Bow, Cheapside, were exhibited. This seal bears date 1598, as mentioned by Stowe, and consists of the spire of the church as it appeared before the fire of London: it is deemed the only parish seal now in existence that was in use in London before that dreadful catastrophe. In the spire there appear places for lanterns, or lights, to direct the good citizens of London their way through the streets at that period, when lamps, owing to the narrowness of the streets, were much more necessary than at present.

The secretary read a most amusing paper on the history, progress, and final conclusion of the cure of the scrofula, or king's evil, by the royal touch. This mode of cure commenced with Edward the Confessor, and, it appears, terminated with Charles I., who is called the antitype of the Confessor. The monarchs of France, too, claimed and actually practised this gift, which Clovis received from our English kings, one of whom *touched* or *cured* (for they were synonimous terms) 92,000 persons. Some additional means were, however, used; and a gold medal was always hung round the neck of the patient, which, if lost, the disease immediately returned: prayers too were made at the ceremony.

The learned secretary quoted some observations on the nature and credibility of testimony in this case by the bishop of Salisbury, applicable to all historical evidence, and highly worthy of attention. It appears that many medical men attested the truth of these royal cures. We trust that the reverend author will present this interesting ac-

count to the public in a more convenient form than in the Archaiologia, that it may serve for an introduction to the history of our modern impostures.

ROYAL ACADEMY.

A general assembly of the academicians was held on Tuesday, the 10th of December, being the anniversary of the institution, when the following were elected officers for the ensuing year:

James Wyatt, esq. president; Henry Thomson, John Hoppner, Thomas Lawrence, Thomas Stothard, Richard Westall, John Francis Rigaud, Richard Cofway, and Edmund Garvey, esqrs., council.

James Northcote, John Hoppner, Henry Thomson, John Opie, Henry Tresham, John Francis Rigaud, Phil. James de Loutherbourg, John Sing. Copley, esqrs, and sir William Beechey, visitors. John Francis Rigaud, and John Soane, esqrs., auditors.

Afterwards the following premiums were given to students, viz.

Painting.—A gold medal to Mr. Douglas Guest, for the best historical picture in oil colours; the subject—bearing the dead body of Patroclus to the camp.

Sculpture.—A gold medal to Mr. William Tolmach, for the best; the subject—chaining Prometheus to the rock.

Architecture.—A gold medal to Mr. William Lockner, for the best design of an elegant villa, with suitable offices, &c. &c.

A silver medal to Mr. Lasc. Hoppner, for a drawing of an academy figure.

A silver medal to Mr. James Noch, for a drawing of an academy figure.

A silver medal to Mr. Richard Tomlinson, for a model of an academy figure.

A silver medal to Mr. J. G. Bubb, for a model of an academy figure.

A silver medal to Mr. William Kinnaird, for a drawing in architecture;

And a silver medal to Mr. James Eames, for a drawing in architecture.

BRITISH INSTITUTION FOR PROMOTING THE FINE ARTS.

We have already announced* the preliminary proceedings for the establishment of this laudable and most useful

* Vol. xxii. p. 88.

institution.

institution. Since that time the Shakspeare gallery, held for a term of sixty-two years from lady-day 1805, under a rent of 125l. a year, has been purchased as an exhibition room for the society for the sum of 4500l. An ample fund has been provided by liberal subscriptions from those of the nobility and gentry who are amateurs and encouragers of the fine arts; and the institution has been regularly organized under the patronage of the king. The following are the bye-laws of the institution.

Patron.—The king.

Vice-patron.—The Prince of Wales.

I. *Of the Object of the Institution.*

1. The primary object of the British Institution, under his majesty's patronage, is to encourage and reward the talents of the artists of the united kingdom; so as to improve and extend our manufactures, by that degree of taste and elegance of design, which are to be exclusively derived from the cultivation of the fine arts; and thereby to increase the general prosperity and resources of the empire. It is conceived, that such an institution is of peculiar importance to the united kingdom at the present moment; when efforts are making in different parts of Europe to promote the arts of painting, sculpture, and design, by great national establishments, and thereby to wrest from us those advantages, which can only be retained by a pre-eminence in the fine arts.

2. With a view to this object it is intended to open a public exhibition, for the sale of the productions of British artists;—to excite the emulation and exertions of the younger artists by premiums;—and to endeavour to form a public gallery of the works of British artists, with a few select specimens of each of the great schools.

3. The exhibition is to be exclusively confined to the productions of artists of, or resident in, the united kingdom; and the higher branches of painting, sculpture, and modelling, are to be considered as the preferable subjects of premiums, and of purchases for the gallery. All other works, however, of the above-mentioned artists will be admissible, if deemed worthy.

4. The British Institution being intended to extend and increase the beneficial effects of the royal academy, which has been founded by his majesty, and by no means to interfere with it in any respect, a favourable attention will be paid to such pictures as have been exhibited at the royal

 academy;

academy; and the British Institution will be shut up during their annual exhibition.

5. The views of this establishment are directed, not only to the promotion of the fine arts, but to the increase of the honour and emolument of our own professional artists; the institution being formed, not as a society of artists, but for their benefit. No subscription will, therefore, be *expected* from professional artists; but their admission will be otherwise provided for. At the same time, if any artist prefers it, he may subscribe in any one of the classes of subscription; and have the same privileges of admission and introduction to the exhibition and gallery, as the other subscribers of the same class; but no one will be capable of being elected on any committee, or of voting as a governor, while he continues to be a professional artist.

II. *Of the Governors.*

1. The government of the institution shall be vested in the present subscribers and contributors of 50 guineas, or upwards, to the funds of the institution, together with such other persons as shall be elected governors as aftermentioned. Those who have subscribed 50 guineas, or upwards, and less than 100 guineas, being governors for life; and those who have subscribed 100 guineas, or upwards, being hereditary governors, their rights being transmissible to their representatives, under the restrictions aftermentioned.

2. All the property of the institution shall be vested in the hereditary governors, subject to the privileges of the life governors, and of the annual and life subscribers.

3. Every governor shall have the right of personal admission to the institution, and of introducing two friends each day to the exhibition and gallery.

4. No governor shall be capable of any office, employment, or engagement, in or under the institution, to which any salary, profit, or emolument, is or shall be annexed.

5. Hereditary shares may be transferable, with the consent of the board of directors.

6. In case of the death of an hereditary governor, his executors or administrators may propose to the board of directors a successor to his share and interest in the institution: and in case such person so proposed shall be a legitimate child of the deceased, such child shall be admitted immediately; but if any other person than a legitimate child shall be proposed, it shall be optional in the board of directors,

to

to elect him or her, or not; and if they do not elect him or her, then they shall pay out of the funds of the institution to such person so proposed, such sum, not less than 100 guineas, as for the time being shall be the qualification of an hereditary governor of the institution.

7. Each governor shall have one vote for every 50 guineas subscribed.

8. Such of the royal family as shall honour the institution by being governors, may vote by proxy.

9. Ladies, who shall be governors, may also vote by proxy.

10. As soon as it shall be found expedient, application shall be made to his majesty, for a charter of incorporation for the institution.

III. *Of the Committee of Directors.*

1. All the affairs and concerns of the institution shall be managed by a committee of directors, to be elected by and from among the governors; and to consist of the president, four vice-presidents, and twelve other members; and three directors shall be competent to business.

2. The president shall be elected annually, and shall preside at the general courts, and at the meetings of the committee of directors.

3. One vice-president and three other members of the committee shall go out annually by rotation; but they may be re-elected, if the annual court thinks fit.

4. The directors shall have power to admit such pictures, statues, and other works of art, as they shall think proper, into the exhibition; and to fix the time of their continuance there upon sale, and to make such orders respecting the admission and sale of pictures, and other works of art, as they shall think fit.

5. They shall have power, from time to time, to make such regulations respecting their own meetings, and the conduct of business therein, as they shall think fit, so as such regulations be not contradictory to the by-laws of the institution.

6. The directors shall have power, with the consent of the visitors, and with the approbation of the governors, to make by-laws for the institution.

7. The directors shall, with the approbation of the governors, fix the hours and times for the opening of the exhibition and gallery, for the governors, honorary members, and subscribers.

8. They shall have power to order tickets of admission,

under such limitations as they shall deem expedient, to artists who are not royal academicians or subscribers.

IV. *Of the Committee of Visitors.*

1. The committee of visitors shall consist of four vice-presidents, a treasurer, and twelve other members, to be elected by and from among the governors; and three visitors shall be competent to business.

2. One vice-president, and three other members of this committee, shall go out annually by rotation; but they may be re-elected, if the annual court thinks fit.

3. The treasurer shall be elected annually.

4. The committee of visitors shall have the power of inspecting and examining the exhibition and gallery, and all other parts of the institution, and reporting their opinion thereon to the general court.

5. They shall audit and examine the annual accounts of the institution, and report thereon, and on the state of the institution, to the annual meeting.

6. They shall have the power of consenting to by-laws.

7. They shall make such regulations respecting their own meetings and proceedings, as they shall think fit.

8. They shall annually elect their own chairman, and deputy chairman, from their four vice-presidents, and their own secretary from the twelve other members of the committee.

V. *Of the Election of Governors.*

1. The qualification of a life governor shall be not less than 50 guineas; and that of an hereditary governor, not less than 100 guineas.

2. Every candidate for election as a governor, must be proposed by a director at one meeting of directors, and at the next meeting of the directors the election may take place.

3. The election of governors shall be by ballot, if required; and no person can be elected, unless two-thirds at least of the directors present vote in favour of his election.

4. In case the person so elected shall not pay in his qualification before his election, or within one kalendar month after, such election shall be absolutely void.

5. In case any person, who shall have paid in his qualification as a governor, shall not be elected, he shall have his qualification repaid him, if he shall require it at any time within two kalendar months after he shall have notice that he is not elected.

VI. *Of*

VI. *Of the annual and other Courts.*

1. The annual court for election of the president, vice-presidents, treasurer, directors, and visitors, and for receiving the annual report of the visitors, shall be held on the Tuesday preceding the 4th of June, precisely at one o'clock, P. M.; and the persons then elected, shall enter on their respective offices on such 4th of June.

2. The ballot for election shall commence at two o'clock, P. M., and continue open till three o'clock; two scrutineers being previously appointed by the chairman, to examine the lists, and to declare the result of the ballot.

3. When, at any election, the votes shall be equal, the chairman shall have a double or casting vote.

4. Special courts may be called by the president, or in his absence by a vice-president, at the request of the committee of directors, or upon the requisition in writing of thirteen governors.

5. No court shall be competent for the transaction of business, unless nine governors be present; and if there shall not be nine governors present, the court may be adjourned for any time, not less than seven days, notice thereof being sent to the other governors.

6. The president, or in his absence one of the vice-presidents, or if no vice-president, one of the governors present, shall preside at the annual and other courts.

7. When, at any court, the votes for and against a question are equal, the consideration of the question shall be postponed till the next meeting.

8. If the business of any court be not completed on the day of the meeting, such court may adjourn from day to day, until the business is completed.

VII. *Of honorary Members.*

1. Honorary members may be elected by the directors, and shall have the privilege of personal admission to the exhibition and gallery.

2. The president of the royal academy, for the time being, shall be an honorary member of the institution, and every other member of the royal academy shall be presented with a silver medal, which will entitle him, or her, to personal admission to the exhibition and gallery.

3. No person shall be capable of being elected an honorary member, except foreign ministers and foreigners of high rank or distinction.

VIII. *Of the annual and life Subscribers.*

1. Subscribers of five guineas a year or upwards, or of fifty guineas or upwards in one sum, shall have personal admission, and the right of introducing two friends each day to the exhibition and gallery.

2. Subscribers of three guineas a year, or of thirty guineas in one sum, shall have personal admission, and the right of introducing one friend each day to the exhibition and gallery.

3. Subscribers of one guinea a year, or of ten guineas in one sum, shall have personal admission to the exhibition and gallery.

IX. *Of the Treasurer.*

1. The office of the treasurer is honorary, and without emolument. He is not to keep any money of the institution in his own hands, but to leave it with the bankers appointed by the directors.

2. He is to pay such sums of money, and no other, as shall be ordered by the committee of directors.

3. He is to give security to the directors, with two proper sureties, in the sum of 5000l. for duly accounting for, and paying into the hands of the bankers appointed by the institution, or otherwise as the directors shall appoint, all monies that shall come to his hands, on account of the institution.

4. He shall keep a general cash-book of all his receipts and payments, which shall be laid on the table of the committee of directors, at all their meetings.

5. He shall make up his accounts to the 31st day of December every year, and lay them before the managers, annually, at their first meeting in the month of February following, in order to their being audited and laid before the visitors, so as to be reported to the annual court.

X. *Of the Officers of the Institution.*

The secretary, keeper, collector, and necessary attendants, shall from time to time be appointed, and their duties and salaries fixed by the directors, and reported by them to the general court.

XI. *Of the Receipt and Expenditure.*

1. One moiety of all subscriptions of fifty guineas, or upwards, together with all legacies, shall be permanently invested in the public funds. The other moiety, together with all other subscriptions and monies received, shall be applicable to the payment of the annual expenses of the institution

institution, and in premiums for young artists; and after retaining what is necessary for payments due, and for current expenses, the surplus shall be annually applied in the purchase of pictures, statues, models, and casts, for the gallery, or in addition to the permanent stock of the institution.

2. No sale, mortgage, incumbrance, or disposition of any freehold or leasehold property of the institution, or of any of its permanent stock shall be made, except with the approbation and concurrence of a general court, summoned with eight days previous notice.

The following circular letter, with an inclosure of the by-laws, and list of subscribers, has been sent round to such persons as were thought likely to promore the objects of the institution:

"The enclosed is submitted to your consideration by the undersigned, who have been appointed a select committee to manage the concerns of the institution, until a committee of directors is elected. Convinced that the pre-eminence, which the imitative arts attained in certain distinguished periods of antient Greece and modern Italy, was produced, not by fortuitous circumstances, but by great and splendid patronage; and persuaded that our own countrymen are capable of the same excellence in the arts, as they have attained in every branch of science and literature, we solicit that they may be encouraged to consider those excellent and immortal examples of the Grecian and Italian schools, as the objects, not merely of imitation, but of competition. In a country where native energy is most abundant, we ask that professional taste and talent, and national patronage, be no longer confined to inferior objects; but that our artists may be encouraged to direct their attention to higher and nobler attainments; to paint the mind and passions of man, to depicture his sympathies and affections, and to illustrate the great events which have been recorded in the history of the world.

"The fine arts are entitled to respect and reward, not simply on account of the innocent and intellectual gratification which they afford, nor merely because they cultivate and civilize the human mind. In a country like our own, they essentially and abundantly contribute to the national prosperity and resources. It must be obvious that the present flourishing state of our manufactures and export trade, is greatly owing to the progress of the fine arts under his majesty's judicious patronage; and that in hardware, cot-

ton, and porcelain, and in every other article to which the industry and attention of the British artisan has been applied, superior beauty of form, and refined elegance of ornament, have contributed to make our manufactures coveted throughout the world, and to introduce them into every country, in despite of political warfare and penal prohibition.

"This preeminence, however, cannot be retained, but by the assiduous cultivation of the fine arts. From those original sources the taste of the country must be cherished and renewed; so as at the same time to produce inexhausted variety, and to preserve classical beauty and chastity of design. And it is, in this respect, worthy of observation, that if we do not advance, we must recede; and that when we cease to improve, we shall begin to degenerate. These considerations are of increased importance at the present moment, when it appears to be the object of other powers, to form great establishments for painting and sculpture, and to extend, by the arts of peace, the influence which they have acquired in war. We feel, however, no apprehension, but that the spirit of the British artist will be awakened and invigorated, whenever a free and fair scope shall be given to his talents;—whenever he shall be stimulated by the same patronage as that which raised and rewarded the Italian and Grecian masters;—a patronage, without which, if we refer to historical evidence, we shall find that no high excellence in art has ever been obtained, in any age, or in any country.

"Under these impressions, we beg leave to address you, and the other protectors of the fine arts, on the establishment of the British Institution; the principal object whereof is to encourage and reward the talents of the artists of the united kingdom, and to open an exhibition for the sale of their productions. For the attainment of this object we earnestly solicit your assistance and patronage; requesting to be favoured with your directions, in which of the classes of subscription you will permit your name to be enrolled. "We have the honour to be,

your obedient humble servants,

Dartmouth.	Abraham Hume.
Abercorn.	Francis Baring.
Lowther.	R. P. Knight.
Mulgrave.	Thomas Hope.
Isaac Corry.	William Smith.
Charles Long.	Thomas Bernard."
George Beaumont.	

The

The committee of directors consists of the following governors:

The Earl of Dartmouth, president.

For one year.

The marquis of Abercorn, vice-president.
Lord Northwick.
Sir George Beaumont, Bart.
Thomas Bernard, esq.

For two Years.

The marquis of Stafford, vice-president.
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Thomas Hope, esq.

For three Years.

The earl of Egremont, vice-president.
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Charles Duncombe, esq. M. P.
Charles Wall, esq.

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The earl of Aylesford, vice-president.
Lord Dundas.
George Hibbert, esq.
Caleb Whitefoord, esq.
William Morland, esq. M. P. treasurer.

LIV. *Intel-*

LIV. *Intelligence and Miscellaneous Articles.*

LETTER ON MALLEABLE ZINC.

SIR,

I AM induced to request your insertion of the following notice, in consequence of having read in your magazine for October *, an account of Messrs. Hobson and Silvester's discovery of a method of making zinc ductile or malleable. I do not mean at all to question that the above-named gentlemen did really of themselves make the discovery of this property of zinc; yet, as I am acquainted with one who has a prior claim to the discovery, I think the public should be informed of the fact, as the reputation of first inventor is often the only reward an ingenious man has for his labour. About twenty years ago, Mr. W. E. Sheffield, of Somers Town (an eminent metallurgist), making an assay of some blende, and being rather impatient to examine the metal, struck an ingot with an intent to break it, while it was yet hot; but was greatly surprised to find, that instead of being brittle and breaking with the usual fracture of zinc, it was extremely tough, and when broken (after many bendings back and forward) exhibited a steel-grained fibrous texture. Doubting whether his metal was only zinc, he repeated the experiment, and with some that he knew to be pure, and had the same result; from which he immediately concluded that zinc, at a certain temperature, was probably as malleable as the other metals. This he found to be the case both in drawing it into wire and laminating it between rollers: specimens of this last, not the 200th of an inch thick, possessing the strength and tenacity of silver, I had from him for a particular purpose, long before the annunciation of Messrs. Hobson and Silvester's patent. Mr. Sheffield has long been in the habit of furnishing the cabinets of his mineralogical friends, both at home and abroad, with a variety of specimens of manufactured and laminated zinc.

I am, sir, yours,
W. LOWRY.

57, Tichfield-street,
Dec. 23, 1805.

CHEMISTRY.

Mr. Parkes, manufacturing chemist, has for some time past been engaged in preparing an elementary chemical work, in a catechetical form, for the use of schools, and for the instruction of those persons who are unacquainted

* Page 93 of the present volume.

with

with chemical science. And in order to remove in some measure the difficulties which such persons find in acquiring chemical knowledge, we understand that he has observed the utmost simplicity of language and arrangement, and has varied the mode of putting the questions, whenever the subjects seemed to require more than ordinary elucidation. A very copious collection of Notes will be subjoined for the use of the preceptor in explaining the doctrines taught in the body of the work, and for the purpose of pointing out the connection which subsists between chemistry and the arts, and showing the various ways in which the several substances in nature are applied in the manufactures of the country. We understand that he intends to annex a vocabulary of chemical terms, various useful tables, a chapter of amusing experiments, and references to the most approved treatises in every department of the science. The work (now in the press) will be ready for publication the beginning of February. It will be entitled a Chemical Catechism, and will be comprised in one volume, octavo.

GALVANISM.

Doctor Joseph Baronio has published at Milan the description of a Galvanic pile, formed of vegetable materials only. The author cut disks of horse-radish and beet-root of about two inches in diameter; aftewards he prepared equal disks of walnut-tree wood. The latter disks are so raised at their edges as to contain a little solution of acidulous tartrite of potash in vinegar, in which they have been previously boiled, to purge them from the resinous principle which the walnut-tree contains. By forming the pile with sixty pairs of disks, one of horse-radish, the other of beet-root, with disks of wood between the pairs, and in each of these a little of the abovementioned solution, he obtained Galvanic effects, in a prepared frog, of which, by means of a leaf of Cochlearia, he made the spinal marrow communicate with the base of the pile, while by a double band of gray paper, well moistened with vinegar, he made its muscles communicate with the top of the pile.

The description of the apparatus is so clear and detailed, as to prove that the author, who is already known by several productions in physical science, wishes that philosophers and amateurs should repeat his experiment.

For horse-radish and beet-root, the author has already substituted disks of other vegetables with equal success, and he flatters himself that these will serve to extend the

application

application of the theory of Galvanism to universal vegetation.

Experiments, which have been made conformable to the theory of M. de Vassalli-Eandi, viz. that there is a development of Galvanism as often as there is a change of capacity, or a chemical mutation in bodies, and to those of Dr. Gardini, of the professors Vassalli-Eandi and Balbis, who have obtained Galvanic effects from plants by applying conductors to the branches and the roots; the experiments also which are conformable to those of professor Rossi, who has formed with success Galvanic piles with vegetables, particularly with sensitive plants and with animals, especially the cold-blooded, without the intervention of any metal; those experiments, with several others made by the academicians of Turin, have induced some philosophers to suspect that the Galvanic fluid is one of the component parts of a natural fluid spread through all bodies in nature, which is put into motion by the chemical action of substances in a state of decomposition, and that it concurs with the other components of the natural fluid to the formation and preservation of all bodies.

ASTRONOMY.

Table of the right Ascension and Declination of Ceres, Pallas, and Juno, for January 1806.

	Ceres.		Juno.	
1806.	A.R. h m s	Dec. N. ° ′	A.R. h m s	Dec. S. ° ′
Jan. 3	7 20 0	29 15	11 41 52	2 37
6	7 16 52	29 31	11 42 52	2 37
9	7 13 44	29 46	11 43 44	2 36
12	7 10 36	30 0	11 44 24	2 33
15	7 7 40	30 13	11 44 48	2 28
18	7 4 44	30 25	11 45 0	2 21
21	7 2 4	30 35	11 45 0	2 12
24	6 59 28	30 45	11 44 44	2 2
27	6 57 12	30 53	11 44 16	1 49
30	6 55 8	31 0	11 43 32	1 35

Pallas is too low to be seen.

NEW COMET.

On Sunday the 8th of December, about six o'clock in the evening, Mr. Firminger, the assistant at the royal observatory, Greenwich, discovered a comet in the constellation Aquarius, a little to the east of south. Its appearance to the naked eye was similar to a star of the first magnitude, when covered by a cloud, through which it might be faintly seen; or rather like what Jupiter would appear under similar circumstances: but when viewed through a night-glass, it appeared to have a bright nucleus surrounded by a coma.

As it was approaching the meridian when first discovered, Mr. Firminger made preparations to take its transit, and found that its light was sufficiently strong to enable him to illuminate the wires in the focus of the telescope, so as to observe its passage with great accuracy.

The mean time of its transit was $6^h\ 24'\ 7''$, with right ascension $11^s\ 23^\circ\ 6'\ 49''$, and south declination $23^\circ\ 41'\ 8''$.

On the following evening it was looked for again; but though the sky was very clear, it could not be seen. It is therefore very probable it may be moving towards its perihelion, and should this be the case, astronomers may expect to find it again in its return from the sun.

The same comet was, we understand, observed by Dr. Herschel, at Slough, near Windsor, about the same time that it was discovered by Mr. Firminger.

COMPOSITION OF MURIATIC ACID.

In our present number (see page 257) is inserted a third communication from Mr. Peel of Cambridge, on the production of muriatic acid and alkalis, by the decomposition of water. Here we have to state the result of an experiment on the same subject by another gentleman.

By continuing to pass the Galvanic fluid from a trough holding about forty pair of square inch plates through distilled water contained in a glass tube, furnished at one end with a wire of gold, and at the other with a wire of platina, at length a coating of oxide of gold was deposited on the gold wire—evidently proving that oxygenated muriatic acid had been formed by the process.

If we are rightly informed, this experiment was performed by Mr. Cuthbertson—as fit a person as any in the world for experiments of this sort.

CURE FOR THE DROPSY.

The following letter has been received by the editor of the Reading Mercury, from Mr. T. H. Shrimpton, governor of the house of industry at Faringdon, dated August 30, 1805:—"In your paper a few weeks since I observed that bohea tea, and the leaves to be eaten, was recommended as a cure for the dropsy; and as I had a pauper in the house at the time, I ventured the experiment, and, to my astonishment, found an almost instant relief. I repeated the dose but once; and the woman, in the course of a week, was able to go out to haymaking, and will begin reaping for me next week, if the weather continues fine. The woman's name is Elizabeth Austin, and her age is 62 years."—The recipe alluded to above is as follows:—Infuse two large teacupfulls of the tea in about a quart of water: let the decoction be drunk during the day, and the leaves eaten at short intervals.

Since the above made its appearance in different newspapers, another instance of the good effects of the prescription has been published:—A woman in Anderston, in the neighbourhood of Glasgow, who has been afflicted with a dropsical complaint since the month of June, has received great relief, and there is a fair prospect of a cure, from eating the leaves of Bohea tea.

CRANIOGNOMY.

Doctor Gall, the craniographic lecturer, is now giving lectures in Denmark, and counts among his approvers in Copenhagen, the philosophers Kallisin, Scheele, Fenger, and others.

TO THE EDITOR.

SIR,

Dining a few days ago with a friend, and being asked the reason why potatoes were sweet after being frosted, I found myself somewhat at a loss to give a satisfactory answer. If any of your correspondents, through the medium of your useful magazine, can give a rational description of the process by which the effect of cold disengages the saccharine principle of potatoes, more particularly than of other vegetable roots, it will very much oblige a constant reader, and may perhaps be useful to some more important purpose in domestic or rural œconomy.

Bedford-row,
December, 1805.

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To John De Lafons, of Threadneedle-street, in the city of London, watchmaker; for a marine alarum chronometer, for ascertaining the time of a ship's log line running out, the time of the watches on ship board, and many other useful purposes. Dated November 19.

To George Wyke, of Winsley, in the county of Wilts, esq.; for a method of working pumps of various descriptions by machinery, whereby much manual labour will be spared. Dated November 19.

To Richard Brown, of the parish of St. Botolph, Bishopsgate, in the city of London, cabinet-maker; for improvements in the construction of several parts of tables, and of various other articles of household-furniture, which stand upon or are supported by legs or feet. Dated November 26.

To James Ingram, of Castle-street, in the city of Bristol; for a new method of manufacturing powder sugar from raw sugar alone, and from the mixtures of raw sugar and syrup of sugar. Dated November 26.

To Samuel Annoss, of Red-Lion-place, in the parish of St. Sepulchre, in the city of London, china enameler; for improved methods of preparing various enamel colours, and of applying the same so prepared to the ornamenting of useful vessels of glass. Dated November 26.

To Joseph Steel, of Stockport, in the county of Chester; for cloths, fustians, calicos, cambrics, lawns, striped cotton, and other articles manufactured with cotton, wool, and flax, mixed and spun together. Dated December 17.

METEORO-

METEOROLOGICAL TABLE
BY MR. CAREY, OF THE STRAND,
For December 1805.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Nov. 27	41°	44°	40°	30·12	7°	Foggy
28	36	43	42	29·90	8	Fair
29	47	54	51	·50	5	Showery
30	51	54	50	·25	0	Rain
Dec. 1	44	45	37	·19	0	Rain
2	34	37	34	·75	15	Fair
3	35	41	47	·91	0	Rain
4	47	52	47	30·00	0	Rain
5	44	47	46	·25	10	Fair
6	50	52	50	·15	6	Cloudy
7	50	53	48	29·90	0	Rain
8	44	45	41	·80	12	Fair
9	44	41	33	·19	0	Rain
10	34	39	32	·20	6	Fair
11	30	37	31	·55	10	Fair, with snow at night
12	32	34	24	·30	5	Cloudy
13	23	28	29	·60	10	Fair
14	29	33	33	·59	0	Snow
15	33	37	32	·80	5	Cloudy
16	27	32	28	30·12	11	Fair
17	25	31	29	·38	7	Cloudy
18	34	37	35	·25	6	Cloudy
19	35	42	44	29·92	8	Rain
20	46	49	48	·50	0	Rain
21	49	52	44	·03	0	Rain
22	39	46	38	28·85	6	Fair
23	34	37	31	29·50	6	Fair
24	36	37	32	·65	7	Fair
25	33	41	40	·74	0	Rain
26	44	46	37	28·95	0	Rain

N. B. The barometer's height is taken at noon.

ERRATA.—In our last Number, page 108, line 10, for Plate IV. read Plate VI.; page 137, line 21, for Plate V. read Plate IV.

LV. *Abstract of Observations on a diurnal Variation of the Barometer between the Tropics.* *By* J. Horsburgh, *Esq. In a Letter to* Henry Cavendish, *Esq. F.R.S.**

SIR, Bombay, April 20, 1804.

When I was in London at the conclusion of the year 1801, I had the pleasure of being introduced to you by my friend Mr. Dalrymple, at which time he presented you with some sheets of meteorological observations, with barometer and thermometer, made by me in India, and during a passage from India to England.

Being of opinion that few registers of the barometer are kept at sea, especially in low latitudes, I have been induced to continue my observations since I left England, judging that, even if they were found to be of no utility, they might at least be entertaining to you or other gentlemen who have been making observations of a similar nature.

During my last voyage I have employed two marine barometers, one made by Troughton, the other by Ramsden; and a thermometer by Frazer. These were placed, exposed to a free current of air, in a cabin where the basons of the barometers were 13 feet above the level of the sea.

The hours at which the heights of the barometers and thermometers were taken, viz. noon, four hours, ten hours, twelve hours, sixteen hours, and nineteen hours, were chosen, because at these times the mercury in the barometer had been perceived to be regularly stationary between the tropics by former observations made in India in 1800 and 1801. It was found that in settled weather in the Indian seas, from eight A. M. to noon, the mercury in the barometer was generally stationary, and at the point of greatest elevation; after noon it began to fall, and continued falling till four in the afternoon, at which time it arrived at the lowest point of depression. From four or five P. M. the mercury rose again, and continued rising till about nine or ten P. M., at which time it had again acquired its greatest point of elevation, and continued stationary nearly till midnight; after which it began to fall, till at four A. M. it was again as low as it had been at four in the afternoon preceding; but from this time it rose till seven or eight o'clock, when it reached the highest point of elevation, and continued stationary till noon.

Thus was the mercury observed to be subject to a regular elevation and depression twice in every twenty-four hours in

* From the *Transactions of the Royal Society* for 1805.

in settled weather; and the lowest station was observed to be at about four o'clock in the morning and evening. I remarked that the mercury never remained long fixed at this low station, but had a regular tendency to rise from thence till towards eight in the morning and about nine in the evening, and from those times continued stationary till noon and midnight.

In unsettled blowing weather, especially at Bombay during the rains, these regular ebbings and flowings of the mercury could not be perceived; but a tendency to them was at some times observable when the weather was more settled.

In the sheets which I formerly presented to you were evinced these elevations and depressions twice every twenty-four hours within the tropics, in steady weather, as had been observed by Messrs. Cassan and Peyrouse, by Dr. Balfour of Calcutta, and others. But since my last arrival in India I have observed that the atmosphere appears to produce a different effect on the barometer at *sea* from what it does on *shore*.

As I am ignorant whether this phænomenon has been noticed by any person before, I will here give you an abstract of my journal, showing how the barometer has been influenced during the whole time since I left England, which will enable you to form an idea whether I am right in concluding that the barometer is really differently affected at sea from what it is on shore, at those places in India where the observations have been made.

The first sheet begins with the observations made on board ship, in my voyage from London towards Bombay, in the months of April and May 1802.

From the time of leaving the Land's End, April 19th, the motion of the mercury in barometers was fluctuating and irregular until we were in latitude 26° north, longitude 20° west, on April 29th; the mercury in barometers then became uniform in performing two elevations and two depressions every twenty-four hours, (which for brevity in mentioning hereafter I will call equatropical motions.) From latitude 26° north to latitude 10° north, the difference of the high and low stations of the mercury in the barometers was not so great as it was from latitude 10° north across the equator, and from thence to latitude 25° south. Within these last-mentioned limits the difference of high and low stations of the mercury in the barometers was very considerable, generally from five to nine hundred parts of an inch, both in the daily and nightly motions.

When

When we reached the latitude of 28° south, longitude 27° west, June 7th, the mercury in barometers no longer adhered to the equatropical motions; but then, as in high north latitudes, its rising and falling became irregular and fluctuating during our run from latitude 28° south, longitude 27° west (mostly between the parallels of 35° and 36° south), until we were in latitude 27° south and longitude 51° east, on the 11th of July. The mercury then began to perform the equatropical motions, and continued them uniformly during our run from the last-mentioned position, up the Madagascar archipelago, across the equator, until our arrival at Bombay July 31, 1802.

August 6th, 1802. When the barometers were placed on shore in Bombay, the mercury, for the first six days, appeared to have a small tendency towards performing the equatropical motions, but not equally perceptible as when at sea, the difference between the high and low stations of the mercury in the barometers being great to the day we entered the harbour of Bombay. From the 12th of August to the 22d, the mercury could not, in general, be observed to have any inclination to perform the equatropical motions, although, at times, a very small tendency towards performing them might be perceived.

On the 23d of August the barometers were taken from the shore to the ship. Immediately on leaving Bombay harbour, August 26th, 1802, the mercury in the barometers performed the equatropical motions, and continued them, with great uniformity, during our passage down the Malabar coast, across the bay of Bengal, in the strait of Malacca, and through the China sea, until our arrival in Canton river on the 4th of October. When in the river, the mercury became nearly stationary during the twenty-four hours, except a small inclination at times towards the equatropical motions, but they were not near so perceptible as at sea; this change taking place the day we got into the river.

During our stay in China, the barometer on shore, at Canton, had very little tendency towards the equatropical motions throughout the months of October and November that we remained there. At times, while in China, a small inclination towards performing the equatropical motions appeared; but, as in Bombay, the difference of rise and fall was of so small a quantity as to be frequently imperceptible.

December 2d, 1802. On our departure from Canton river the equatropical motions were instantly performed by the

 mercury,

mercury, and with great regularity continued during the whole of the passage to Bombay, until our arrival in that harbour on the 11th of January 1803.

On January 18th the barometers were placed on shore, and did not appear in the smallest degree subject to the equatropical motions; although, with great regularity, they had been performed while at sea, even to the day we entered the harbour. One of the barometers was left on board for a few days, and, like that on shore, seemed to have no tendency towards the equatropical motions. During the months of February and March, in Bombay, the mercury was nearly stationary throughout the twenty-four hours. But about the latter part of March the mercury seemed to incline towards the equatropical motions in a very small degree; and, during the month of April, and to the 20th of May, this small tendency of the mercury to perform the motions appeared at times, but was hardly discernible, the rise and fall being of so small a quantity. From the 18th of January to the 20th of May the mercury in the barometers was in general stationary, except a very small tendency towards the equatropical motions at times. At other times some change in the atmosphere disturbed the mercury from its stationary position: but this was seldom the case, as it was then the fair weather season, or north-east monsoon.

We sailed from Bombay on the 23d of May 1803. The instant we got out of the harbour the mercury in the barometers conformed to the equatropical motions with great regularity, and the difference between the high and low stations was very considerable during the whole of the passage to China, excepting a few days in the eastern parts of Malacca strait, where the land lay contiguous on each side of us: the difference between the high and low stations of the mercury was then not so great as in the open sea. On clearing the strait, and entering the China sea, the equatropical motions were performed in greater quantity, and continued regular during our passage up the China sea, until July 2d, 1803. We then entered Canton river, and the equatropical motions of the mercury in barometers entirely ceased.

From July 8th to September 7th, the barometers were placed on shore in Canton, during which time the mercury appeared to have no tendency towards performing the equatropical motions; but it inclined to a stationary position, except when influenced by changes of weather. After the barometers were taken from Canton to the ship, we were

four

four days in getting clear of the river, in which time the mercury inclined to be stationary, excepting that a small inclination towards the equatropical motions seemed to evince itself at times. But no sooner had we cleared Canton river, September 13th, 1803, than the mercury in the barometers began to conform to the equatropical motions, of two elevations and two depressions every twenty-four hours, at equal intervals of time, (although we were near the land until the 15th of September.) And the mercury, with great regularity, continued to perform the equatropical motions from September 13th, 1803, the day we cleared the river of Canton, until October 13, when we entered Sincapore strait, excepting a small degree of irregularity, which affected the mercury on the 22d of September, when it blew a gale on the coast of Isiompa.

October 13th, 1803. On entering the strait of Sincapore, which is about three leagues and a half wide, the mercury in the barometers was then a little obstructed, and did not perform the equatropical motions in the same quantity of rise and fall as when we were in the China sea. But on the following day, October 14th, when we had passed the narrow part of the strait, the mercury conformed to those motions with regularity until October 21st, when we arrived in the harbour of Prince of Wales's Island: then a great retardation took place in the equatropical motions; for, during the time the ship remained in the harbour, from October 20th to November 5th, 1803, the mercury in barometers seemed only in a small degree subject to them, the difference between the high and low stations of the mercury being in general not more than half the quantity that takes place in the open sea, or at a considerable distance from land. Where the ship lay at this time in the harbour, the land, on one side, was a full quarter of a mile distant, and on the other side about a mile and a half.

On November 5th, being clear of the harbour of Prince of Wales's Island, the equatropical motions were instantly performed by the mercury in the usual quantity experienced at sea, which continued with uniformity until December 3d. On this and the following day the mercury fell considerably during our passage over the tails of the sands at the entrance of Hoogly river in latitude 21° 06′ north; and on December 5th, the day of the moon's last quarter, a gale of wind commenced from north-north-east, with much lightning and rain in the night. During the latter part of this day the mercury began to rise, and there soon followed a change of settled weather. When we were in the lower part of

 the

the river the mercury appeared to conform in a small degree to the equatropical motions; but when well up the river, at Diamond harbour, the mercury inclined to be nearly stationary during the twenty-four hours, as has formerly been observed to happen in Canton river, Bombay harbour, &c.

On January 13th, 1804, after we had cleared the river Hoogly, the mercury in the barometers began to perform its motions with uniformity, which continued during the passage to Bombay until our arrival there on February 12th. The barometers being then placed on shore, the mercury inclined to a stationary position, without evincing any propensity towards the equatropical motions from the 12th to the 18th of February 1804, as has been noticed in the foregoing description to happen frequently on entering a harbour from sea.

On February 18th, 1804, the meteorological journal ceases, at which time it comprises the observations of twenty-two months, having commenced April 6th, 1802, in Margate Road.

I have taken the liberty of sending you this abstract from the journal, to exhibit the apparent difference of the mercury in the barometer at sea, from what has been observed on shore, at those places mentioned in the preceding description. As I have not seen any account indicating the phænomenon, I thought it might be interesting to you, or other gentlemen of the Royal Society, to forward this imperfect abstract, the journal itself being too cumbersome to send home at present. But as I am in expectation of returning to England by the ships from China next season, I hope I shall be enabled to present you with the meteorological sheets alluded to above.

I am, &c.

J. Horsburgh.

P. S. Since I wrote the foregoing abstract I have received a letter from my friend Mr. Dalrymple, intimating that a copy of the meteorological journal itself would be acceptable; which has induced me to transmit to him the original sheets, with a request to deliver them to you. I regret that I could not find leisure time to make out a fair copy to have sent to you, in place of the original sheets in their rough state.

Bombay,
June 1, 1804.

LVI. *An*

LVI. *An experimental Inquiry into the Nature of Gravelly and Calculous Concretions in the Human Subject; and the Effects of Alkaline and Acid Substances on them, in and out of the Body.* By THOMAS EGAN, *M.D. M.R.I.A.*

[Continued from p. 212.]

THESE facts being pretty well established and acknowledged, it is time to inquire how far we may account for them; and whether experiments, instituted out of the body, may not throw some light on this subject. Dr. Saunders, in a letter to Dr. Percival, (Percival's Essays, Medical and Experimental, vol. iii.) on the subject of carbonic acid as a solvent of calculous concretions, observes, "If a more powerful and active solvent than any hitherto known shall be discovered, it is highly probable that such a discovery can only be made by a rational and chemical inquiry into the powers of different bodies of combining with the contents of the urine, and preserving them in a fluid state out of the body." Now, on the other hand, we may presume, that whatever substances cause a separation or precipitation of uric acid, in an aggregate state, from healthy urine, will give rise to these disorders. For we are not to forget that the uric acid, which forms so large a proportion of calculous concretions, and the entire of the gravelly, is a natural secretion from the blood, performed by the functions of the kidneys, and excreted by the urine, and can only be prejudicial by a previous morbid separation from it within the body. With this necessary view of the subject before us, (for which we are, as already observed, indebted to Boerhaave,) I resolved to try, 1st, What might be the effects of acids of different kinds on healthy urine, as to their influence in causing this same previous precipitation; and, 2dly, that of alkaline substances in preventing it. And here it must be observed, that to draw any satisfactory conclusions from experiments made with these substances out of the body, we must suppose they reach the kidneys and blend with the urine, still possessing their relative distinctive properties; and that this takes place, we have every reason to presume. Doctors Percival and Saunders, Mr. Bewley, and others, have ascertained the presence of carbonic acid, in an uncombined state, in the urine of those who drank the mephitic water for some days: an acid certainly foreign to its recent healthy state; for, after repeated trials, by heating it to nearly ebullition in one of Priestley's air bot-

tles, I never could procure the separation or transition of a single bubble of carbonic acid into a jar of lime water. And if this weak acid reaches the kidneys undecomposed or uncombined, we shall have less difficulty in believing the more powerful ones may do so. That the tartarous acid in the combination of the acidulous tartarite of potash exerts powerful effects on the functions of the kidneys, is well known; and that the urine is at the same time rendered more acid, I have repeatedly ascertained by the usual tests.

We may say the same of the other vegetable acids, which manifest also diuretic powers, and increase the natural acidity of the urine. Linnæus, in his second volume of the *Amœnitates Academicæ, De Genesi Calculi*, already quoted, mentions his having made the following experiment to this purpose. He says: "Hisce diebus ipse experimentum institui cum urina; hæc communiter a solutione lacmus parum admodum rufescit; at si libram unam vel alteram vini Rhenani, vel alterius vini acidi hauserim, post horam unam vel plures, valde rubra et rutilans evadit urina, ab affusa solutione lacmus; certo indicio, acidum vini totum corpus permeasse, et urinam infecisse." Nor should we wonder that these energetic substances should pass unaltered to the kidneys, when we find so many mild vegetable matters do so. I will not mention the communication of so volatile a principle as odour, but will more particularly dwell on that of colour. Rhubarb, turmeric, madder, and many other substances, so completely impart their colour to urine, that they would appear to be very little altered. Nay, the juice of the beta vulgaris, a mild esculent of the pentandrous class, so deeply reddens it as to cause it to be mistaken for bloody urine, of which a late instance has occurred in my practice.

As to alkaline substances, it has been at all times known that they communicate their properties to this excrementitious liquor. A perseverance in the use of the *aqua kali puri* of the shops for a few days, even in small doses, converts its acescent into the alkaline state; and we have every reason to suppose that the same takes place with the carbonates, which are taken in so much larger quantities. This seems confirmed by experiments made in London and Paris; and the alkalescent impregnation of the urine was ascertained by the formation and precipitation of the acidulous tartarite of potash upon the addition of the tartarous acid. Yet, from a good deal of experience in these matters, I may aver, that as to the carbonates the dose must be considerable, (which was the case in London,) and continued for

for some time, having frequently given two scruples of desiccated soda (containing, according to Mr. Kirwan, 23·94 grains,) in the twenty-four hours, for some days together, without any diminution of the usual acidity of this liquor.

For the information of such of my readers as may not be of the medical profession, I must here observe, that physicians distinguish two kinds of urine: the one rendered immediately after meals, and much dilution, before the process of digestion, or state of sleep, can take place; always more or less limpid; being comparatively less charged with the natural component parts of urine, (the urée, or extractive colouring matter, in particular,) and called *urina potus*, to distinguish it from the *urina sanguinis*, rendered many hours after meals and sleep, the taking no more than a necessary quantity of liquids, and containing the usual proportion of saline and other ingredients; more especially the urée, to which it owes its natural citrine colour.

This last, therefore, was that employed in the following experiments, if not otherwise specified; with the chemical history of which I must suppose gentlemen of the profession now tolerably well acquainted, being so fully and accurately detailed in the tenth volume of the *Connoissances Chimiques*.

Having, in the preceding pages, insisted so much on the acids and acescent drinks as occasional causes of these complaints, the first object seemed to be, to ascertain whether the urine of those most subject to them, or actually labouring under them, was more relatively acid. We have already seen, from a register of these patients, kept for forty years in the hospital of Luneville, that the early period of life, from two to six years of age inclusive, is most liable to calculous affections. Now, the urine of healthy children is always found more acid than that of adults, generally in the proportion of two to one. Whilst several drops of the latter are requisite to redden a given quantity of infusion of litmus, a single drop of the former turns it to a clear red. Paper stained with an infusion of turmeric, and reddened by an alkali, was immediately restored to its colour by a single immersion in the urine of children; an effect which required some time in that of adults. And that this should be the case we shall not be so much surprised at, when we consider the nature of their diet; and that, in addition to the phosphoric and uric, their urine contains the benzoic acid in considerable quantity, the proportion of which is found afterwards progressively to diminish with their advancement in life.

The

The constant opportunity I have of attending to those subjects, enables me to say, that the urine of gravelly patients, when fresh rendered, nay, after standing many hours, in a temperature of sixty degrees, is relatively more acid than the healthy, sometimes as much so as the gouty; and frequently continues so, even after depositing its gravelly matter. An exception to this, however, sometimes occurs in gouty habits; their urine depositing copiously this acid substance, and yet manifesting no increased, but sometimes rather decreased, acescency; for with them a considerable diminution of the quantity of the usually excreted superacidulated phosphoric salt often takes place, as shall be fully explained upon another occasion.

Having premised these observations, it is now time to consider what effects acid substances are productive of, when mixed, out of the body, with this very complicated liquor. And here, to prevent repetition, I will observe, that that generally used was rendered fresh in the morning, in the quantity of from three to four ounces, (unless otherwise specified,) being that most easily retained at one time in the bladder. The quantity of acid extremely small, for obvious reasons, and seldom increasing its acescent properties (as ascertained by the usual tests) beyond what frequently occurs in the urine of those who use acescent drinks, or are afflicted with gout or gravel. A standard quantity was always laid by for comparison; and the temperature from sixty to seventy-five degrees, being in autumn 1799. And to begin with the vegetable acids:—

Experiment I.

To four ounces of the urine of an adult was added one drachm of common acetous acid, which, like every other acid, caused no immediate change in it; but in a very short time, and before it cooled down to the temperature of the atmosphere, some extremely minute shining spiculæ, observable only by a lens, were seen floating in it: these gradually increased in number and size, began to reflect the light, and, from being perfectly transparent, soon became coloured, to settle upon the usual cloud, or *nubecula*, which now began to form, adhere to the sides of the glass, and partly fall to the bottom in the shape of small bright red crystals. In the standard, after twelve hours, nothing more observable than the usual *nubecula*; nor was there any sign of crystallization, or separation of uric acid, even after twenty-four.

Experiment

Experiment II.

To the same quantity of adult urine were added one drachm and half of acetous acid, which caused a more copious separation and crystallization of this substance with the foregoing appearances. None observable in the standard after twenty-four hours.

Experiment III.

To four ounces of urine of a healthy child, who never was observed to pass gravel, and of the usual degree of acidity, was added one drachm of acetous acid, which soon caused an evident and copious separation of crystallized uric acid. The crystals were, however, not quite so coloured; the urine of children not being so much impregnated with the urée, or colouring matter. No such appearance in the standard after twelve hours or more.

Experiment IV.

To four ounces of adult urine, rendered very soon after a tea breakfast, and nearly in a state of *urina potus*, was added one drachm of acetous acid. After three hours, a crystallization of minute sandy particles took place. None in the standard, even after three days.

Experiment V.

Thirty drops only, of acetous acid, were added to four ounces of the urine of a gouty patient æt. sixty, and who sometimes felt some slight gravelly tendency. A very copious precipitation of this matter quickly took place. Some observable in the standard, also, the next day.

Experiment VI.

To three ounces of healthy adult urine were added a few drops only of citric acid. A distinct crystallization, but extremely minute, took place. No appearance of any in the standard after many hours. The experiment was repeated with one drachm of filtered citric acid, which only hastened the separation and increased the quantity of crystalline matter.

Finding, by these experiments, and numberless others, with a detail of which it would be unnecessary to take up the time of the academy, that the acetous and citric acids, blended with the urine, separated its uric acid in a crystallized state, I thought it might be interesting to investigate what the effect of the tartarous acid might be, being that which, in an uncombined and partly combined state of acidule, as in

in the acidulous tartarite of potash, chiefly prevails in the wines and beverage of those countries most subject to these complaints.

Experiment VII.

To four ounces of healthy adult urine were added some drops only of pure tartarous acid. To the same quantity one drachm of acetous acid, which brought them nearly to the same standard of acidity; a circumstance always attended to in the comparative trials with different acids. In that with the tartarous acid the crystals were not only larger and darker coloured, but exceeded in quantity any thing before observed. In that with the acetous acid, a much smaller proportion of minute crystals took place.

Experiment VIII.

To four ounces of urine were added two drachms of a filtered solution of acidulous tartarite of potash of the temperature 55 degrees. The usual separation and crystallization took place in large proportion: the crystals, however, much smaller, and less coloured, than those with the uncombined tartarous acid. The two last experiments, frequently repeated, presented the same results.

Experiment IX.

The result of the above experiments having led to some doubt as to the good effects of the carbonic acid gas, so much, at one time, recommended by doctors Percival and Saunders, previous to its more modern alkaline combination in our mephitic as well as super-aërated soda waters:

Into the middle part of Nooth's apparatus were introduced four pounds of fresh rendered healthy urine, and exposed to a stream of carbonic acid gas. After a few hours a copious and beautiful precipitation of uric crystals took place, (notwithstanding the constant agitation from the transmission of the gaseous bubbles,) larger than any I before observed, that from the tartarous acid excepted. In a standard quantity, no distinct crystallization, even after two days. A repetition of the same experiment afforded similar results.

Experiment X.

Finding the carbonic acid gas productive of similar effects with the other acids hitherto examined, it was natural to inquire how far its combination with the portion of alkaline matter contained in our mephitic and soda waters, so highly surcharged with it, may prevent a separation of this uric acid.

Half

Half an ounce only of the common soda water of the shops, prepared by Mr. Kinsley, was added to four ounces of healthy urine. A similar quantity was impregnated with carbonic acid gas. In the former, after forty-eight hours, or more, no more than the usual nubecula; nor could a single crystal be discovered even by a magnifier. In the latter, an early, copious, and beautiful crystallization. On the result of this experiment, frequently repeated, with various proportions of the mephitic alkaline water, I shall afterwards have occasion to make some remarks.

Though the mineral acids, in an uncombined state, enter not into the matter of our diet, and are no longer considered as lithontriptics, since the notion of the earthy nature of these concretions has been abandoned; yet, as they are sometimes prescribed with other indications, I thought fit to extend my researches, though in a summary way, to them also.

Experiment XI.

To sixteen ounces of urine were added eight drops of very dilute sulphuric acid. To a similar quantity, two scruples of citric acid, to bring them to nearly the same standard of acidity. After a very short interval, in that with citric acid, the usual appearances of transparent floating moleculæ reflecting light, and gradually becoming larger, were observed, and began to adhere to the glass; whilst in the other, after five hours, no such appearances took place. Yet, after forty-eight, here also a precipitation took place of smaller crystals, and less in quantity; for, being collected on a filter, and carefully dried, they weighed only two grains, whilst the former amounted to three. And this is nearly the largest proportion I ever found the above quantity of healthy urine to contain.

Experiment XII.

As the nitrous acid is one of the most active solvents of this matter, out of the body, I was curious to ascertain, whether, in the very dilute state in which it must reach the kidneys and bladder, (where its action must have been facilitated by the actual state of solution of this substance,) it would manifest its powers in preventing its separation.

To three ounces of urine, rendered a few hours after breakfast, and, of course, scarcely acid, were added five drops of weak nitrous acid, which did not seem to add very materially to its acescent properties.

To a similar quantity were added four scruples of acetous acid

acid. In less than an hour the former deposited a distinct quantity of gravelly matter in considerable proportion. This, perhaps, we should not be surprised at, when we consider how the action of this acid in that fluid may be determined by superior affinity. In the latter the separation did not take place for a considerable time after. We see, then, that the nitrous acid speedily and powerfully precipitates this acid substance.

Experiment XIII.

To six ounces of urine, showing a strong acescent quality, were added only three drops of strong marine acid. A cloudiness and transparent granular precipitation took place, followed by the formation of extremely minute gravelly concretions, which, even after two days standing, did not assume so red a tinge as that with vegetable acids. This may probably depend upon some action of this acid upon the urée, or colouring matter; but as to the smallness of the crystals, that evidently depends upon the more speedy precipitation, throwing them down before they can assume their natural size, and leaving but a shade of difference between the crystalline and pulverulent deposits.

Experiment XIV.

From the above, then, we are satisfied that the vegetable and mineral acids cause a premature separation and crystallization of the lithic contents of recent healthy urine; but it may be observed that this only takes place under circumstances not at all applicable to the living system, viz. a much inferior temperature, and, in some instances, a contact with the atmospheric air; two powerful promoting causes of crystallization in general, but more especially of the less soluble salts. To determine, therefore, this most essential point:

To six ounces of cold but recent urine (in a well closed phial) were added five drops of very dilute nitrous acid, which were placed on a sand bath: temperature varying from 80 to about 100 degrees at most. The same quantity, with similar precautions, but without addition, was laid aside in the laboratory as a standard: temperature 56 degrees. After a very short interval indeed, and almost as soon as the urine acquired the temperature of between 80 and 90 degrees, small shining granular particles were observable with a magnifier, began gradually to settle upon a broken kind of nubecula or rather nubeculæ, and to acquire colour and size, though carried up and down the liquor, which was in constant agitation. This experiment again twice

twice latterly repeated, and always with the same result, (care being taken to keep the temperature, as nearly as possible, for a few hours, between 90 and 100 degrees,) afforded one of the most pleasing objects imaginable, viz. the formation of this crystalline matter, under all the disadvantages of elevated temperature and constant agitation, from (I may almost say) their primordial moleculæ to the accomplishment of their full size. And here, indeed, they are most beautiful, and not to be distinguished from those spontaneously deposited.

The whole experiment strikes us strongly with a semblance of what probably passes, under similar circumstances, in nature; and reminds us of the danger attendant upon acid impregnations, more particularly at bed-time, when the urine, by many hours retention and quiet, has ample time to deposit its uric acid contents in the bladder. From it also we learn, that the temperature of the human body, in place of retarding or preventing (as might be expected *à priori*) these pernicious effects, rather promotes them, and that to a considerable degree.

But whilst we endeavoured to establish this point, from practical observation as well as experiment, we seem to have entirely forgot that the urine itself is an acid liquor, and that therefore, if acids were so prejudicial, it is not probable that the provident wisdom of nature would commit the discharge of this necessary excretion to a fluid, which, by prematurely separating it within the body, would completely defeat the object of her humane attention. And would she not, in the infinity of her resources, dispose of it by some less objectionable emunctory?

I would, in the first place, observe, that though healthy urine manifests the properties of an acid liquor, it is in the very smallest possible degree; so much so, that though mentioned long since by Moraung, Coldevillars, and other surgeons, yet it was not, either chemically or medically, acknowledged to be so, until the time of Scheele, who finally established this point, as well as the nature of the prevailing acid. And, secondly, that nothing can be more erroneous than the opinion, which so long prevailed, that the phosphoric acid existed in it in a naked or uncombined state. It is now well established that it is only in that of a weak acidule, or acidulous phosphate of lime, very little short, indeed, of the point of saturation; and hence the weakness of its action as an acid liquor: for were it not for litmus, and some of the more delicate of the vegetable blues, we should have been, even to this day, ignorant of

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this property; so very feeble, indeed, that it will often not affect an infusion of red cabbage, whilst it turns with litmus, and sometimes, but feebly, with this most delicate of all acid tests. A single drop of phosphoric acid was added to one ounce of distilled water. Of this weak acid impregnation one drop was sufficient to turn the infusion of litmus of as clear a red as the mineral acids do; whilst seven of urine manifested but very weak effects of acidity, and required some time to show any. If the urine, therefore, does not exceed its natural standard of acidity, we have nothing to apprehend. And here, indeed, we must again admire the wonderful wisdom of Providence. The occasion (may I be allowed to say so, and that, too, before so competent an assembly?) required some chemical discrimination. It was necessary to carefully provide for the expulsion of the recrementitious part of the osseous fabric, which is very considerable, out of the system; but as this salt is insoluble in an aqueous vehicle, such as the urine, nothing more would be necessary to obviate this difficulty than a certain degree of super-saturation, or state of acidule, which would more effectually provide for its solubility and its elimination. But by going thus far, whilst it attended to one excretion only, it would have entirely forsaken its charge of another, committed also to this fluid; and by this degree of super-saturation, precipitate, retain in the system the uric acid, and occasion as frequent an occurrence of gravelly and calculous complaints, amongst mankind in general, as now occurs among the gouty. It therefore prudently formed that degree only of acidulous phosphate of lime, which, though insoluble out of the body, was sufficiently soluble when assisted by its temperature. Nay, even for wise purposes, it has given a degree of latitude to this temperature, which, though narrow and confined indeed, is sufficient for its purposes; but where it precisely terminates I am not at present prepared to say, though so easily determined.

Let us now, for a moment, consider how far any morbid deviation from this healthy standard (which sometimes happens) may throw light on this subject. The most considerable, that I am acquainted with, occurs in the instance of gouty urine rendered towards the decline of the paroxysm. A single drop of this, though in a turbid state, affects the vegetable blues with an energy equal, or perhaps superior, to that of the strongest acetous acid; and requires a very considerable increased proportion of lime water to decompose it, for obvious reasons. This we find always depositing,

depositing, sometimes from the bladder itself, but generally before it has entirely parted with its natural temperature, a very large proportion of a reddish brickdust-like sediment (a welcome harbinger to gouty patients), gradually declining, and keeping pace with the alleviation of symptoms, and the progressive return of the urine to its natural degree of acidity. This sediment, Scheele, Bergman, and Fourcroy, consider of the uric acid kind: and so it (but in part only) undoubtedly is, being in a smaller proportion than they were aware of. For, considering that the enormous quantity, rendered in a few days, was incompatible with the known minute proportion of this acid matter in urine, I was determined to make the following experiment:—To a considerable quantity of it, desiccated and well edulcorated with distilled water, were added three ounces of a weak alkaline lixivium; which, after a few hours digestion, completely discoloured it, acquired a golden yellow colour, a sweetish taste, and, on the addition of a few drops of dilute marine acid, precipitated a copious sediment of whitish, minute, needle-shaped crystals, of a silky appearance.

To this precipitate, well edulcorated, was added, by degrees, about one ounce of weak nitrous acid, which acted on it with effervescence, and nearly took up the whole. This solution, being set to evaporate, began to redden the fingers, and other animal matters; no doubt, therefore, could subsist as to its nature. To the remainder, which seemed very little diminished, and only deprived of colour, were added two ounces of dilute marine acid, which, after some time in digestion, nearly dissolved the whole; and finding this acid solution precipitate with lime water, oxalate of ammonia, and fixed alkali, it must have been phosphate of lime. This forms, then, by far the largest proportion of the gouty sediment, which is coloured by the precipitated uric acid. Such also is the result of Crookshank's experiments; and so we should expect to find it, as I shall endeavour to point out, on a future occasion.

Let us now consider how far these analytical results may be confirmed in the synthetic way, having resolved that experiment, as far as applicable, should form the basis of any opinions offered in this essay. The phosphoric, being the native acid, prevalent in urine, it was interesting to determine, whether, by the artificial super-addition of it, so as to bring this fluid to the standard of the gouty, we might not produce effects somewhat analogous to what occur there.

 Eighteen

Eighteen ounces of urine were divided into three equal parts. To the first were added five drops of sulphuric acid; to the second, ten; and to the third, fifteen. In the first the magnifier very soon discovered minute floating moleculæ, gradually assuming the crystalline form, &c. as so often before described. In the second, the same appearances, but more immediately and copiously produced. But in the third, so considerable as to excite my astonishment. For here, besides the same extremely minute crystals which adhered to the entire sides of the phial, the bottom appeared covered with a mixture of crystalline and red pulverulent matter; the latter in great proportion, and probably prevented from crystallization by its hasty deposition. Here, then, that increased proportion of calcareous phosphate and animal gelatinous matters, (which always takes place in gout, and could not be expected here,) would seem only wanting to form a sort of synthetic approximation to the gouty sediment.

The unusual proportion of deposited uric acid in this experiment created some suspicion that the phosphoric acid might, by a combination with some of the principles of this very compound fluid, give rise to some artificial formation of it on this occasion.

To the filtered liquor, therefore, of No. 3, were again superadded five drops, which in twenty-four hours caused a further separation of a very few crystals only. It was filtered a third time, and eight drops more added; but without the smallest appearance of a single crystal after four days. The additional acid, then, only more effectually and speedily determined the separation of the quantity naturally contained in urine; its more divided pulverulent appearance adding considerably to its volume.

It now only remained to demonstrate the identity of these various precipitates with the naturally deposited matter of gravel. For, though it could not be well mistaken for any other saline composition in urine; yet, as external characters are, even in the hands of a Romé de Lisle, or an abbé Haüy, fallacious, the following, and concluding one, on the subject of acids was instituted.

Experiment XV.

To two drachms of this artificial gravelly matter was gradually added one ounce of nitrous acid, which acted on it with effervescence, and dissolved the whole, with the exception of some small, floating, succulent, animal particles, so well described by Bergman.

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The evaporated solution reddened the skin, and, after some time, deposited crystals of oxalic acid; as happens in all concentrated nitrous solutions of calculi of the uric acid kind. To another small quantity was added some pure alkaline lixivium, which very soon took it up, became coloured, sweetish, and deposited the usual silky crystalline sediment upon the addition of acetous acid. No doubt, therefore, could remain, as to its identity with that naturally deposited.

And here, though irrelevant to my present object, and merely with a view to excite the attention of the faculty, may I be permitted to ask, how it happens, that in the very worst kinds of typhus fever there is very little diminution of the secretion or excretion of the acidulous phosphate of lime? as appears by the acidity of the urine, lime water, and the quantum of precipitate afforded by the oxalic acid; whilst a very considerable one of the uric acid takes place, and continues so until nearly the termination of the disease, when it begins gradually again to manifest itself; first, by the usual tests only, but presently, upon the crisis taking place, in such quantity as to become insoluble; and therefore quickly precipitates (with some additional mixture of calcareous phosphate and animal mucilaginous matter) under the form of our critical sediment or deposit? or, are we not here again to admire the wise œconomy of the Author of nature, which, by keeping up the considerable and necessary bony excretion of the system, prevents the dangerous accumulation of it which must ensue from its retention during the long protracted period of many fevers? I might here offer some conjectures in explanation, but will reserve them for another place.

Having already trespassed so much upon the indulgence of the academy, I shall here content myself with briefly stating, that, from the above experiments and observations, we may presume to say acids of every kind are prejudicial, and give rise to the formation of gravelly and calculous affections, by causing a separation and crystallization of the lithic acid contents of the urine within the body; not pretending, however, to deny the existence of other causes inherent in the system itself, occasionally productive of similar effects, as has been already observed.

I shall now proceed to the second part of this inquiry; namely, how far, or in what manner, alkaline matters are conducive to the alleviation of these complaints.

[To be continued.]

 LVII. *Letter*

LVII. *Letter from* John Pollock, *Esq. of Mountainstown, Navan, to the Reverend Dr.* Lyster, *respecting a Surgical Operation performed on a Heifer* *.

DEAR SIR,

Perhaps the following communication may be thought worth promulgating by the society.

I have bred some very fine heifers of the long-horned Leicestershire breed. One of them lately was seized with a complaint in the throat, attended with a difficulty of breathing, and an apparent difficulty in swallowing; both which symptoms increased, notwithstanding the applications and exertions of all the experienced herds and people near me who had the reputation of skill in curing those disorders to which cows are liable: in a few days the heifer refused to take any food, and, being considered as incurable, she was killed. I had her throat opened, and found that an excrescence of flesh, or polypus, had grown somewhat below the root of the tongue, to the size and nearly of the shape of a kidney, save that it had plainly a neck or root from which it grew, and that this polypus had nearly covered both the throat and windpipe of the animal; and further, that the excrescence was in a general state of putrefaction. I lamented a good deal the loss of the heifer; but in a few days after I was much concerned to observe that another of my stock, of the same breed and age, (now rising four years old,) discovered exactly the same symptoms, and progressively grew worse, being scarcely able to breathe, and refusing all kind of food. I had heard of a surgical operation in cutting out a polypus from the human nose: I at length determined to try the experiment on my heifer: for that purpose I threw the animal down by ropes; and having tied her legs, and got sufficient assistance to keep down her head, I opened, under the insertion of the jaw-bone, a passage by the side of the windpipe completely into her throat (taking great care to avoid the great veins and arteries in the side of the neck) sufficiently large to admit a man's hand. I then made my steward put his hand into the wound and take hold of the polypus, and pull it with some force out of the wound, so that the neck of it was visible. I then cut off the polypus, sewed up the wound, leaving however a seton in it, and untied the heifer: for the remainder of the day she refused

* From *Transactions of the Dublin Society*, vol. iii.

both food and drink: the next morning I had her turned for a few hours to grass, which she began to eat; and from the inclination of her head and neck whilst eating, a most considerable discharge of suppurated matter came from her nostrils. She, however, daily recovered: the suppuration from her nostrils soon disappeared; but the discharge from the wound in the neck continued in a considerable degree, by reason of the seton I had put into it, and which I further encouraged by the application of warm stupings twice a day. The animal, in the course of one week from the time of the operation, recovered perfectly, and is now as well as ever she was. I am, dear sir,

Your most obedient servant,

JOHN POLLOCK.

Rev. Dr. Lyster, &c. &c.

LVIII. *Description of Mr.* DAVID CHARLES'S *Machine for laying Land level. By Lieutenant-Colonel* HARDY*.

SIR,

EVERY new invention that lessens the expense of manual labour must become an object to your society; I therefore beg leave to inclose the plan of an instrument for levelling ground, used by me this last season. Its application is simple, and its success so evident, that two neighbouring farmers borrowed it, and used it in the same manner.

Should the society consider it worth adopting, and that the sketch accompanying this letter is not sufficiently clear, I shall with great pleasure send a model to their repository.

I have the honour to be

Your most obedient servant,

JOSEPH HARDY.

Westmead Langhorne, Carmarthenshire, Jan. 5, 1803.

Mr. Charles Taylor.

This simple machine, which is the invention of my steward, and of which I have seen nothing similar, appears to me necessary, even in the most fertile parts of England, where the new system of drill husbandry has been introduced, or even where there is any attention to

* From *Transactions of the Society of Arts*, &c vol. xxi. The silver medal of the society was voted to Mr. Charles for this invention; and the thanks of the society were given to lieutenant-colonel Hardy for his communication, and for a model of the machine.

the waste of time, or to the ease of cattle in the act of ploughing, in order to get rid of crooked or unequal ridges without either a summer fallow by cross ploughing, or else by frequent repetitions of ploughing in the winter and spring, which the humidity of our climate will not allow in every kind of soil.

I reduced fourteen acres of land last spring to a perfect level, where the crowns of the ridges were above two feet higher than the furrows, and where they were crooked and of unequal breadths. Six acres of this is now under turnips, a crop that gives sufficient time to ameliorate the under strata of soil that had perhaps never before been exposed to the influence of the sun and air; and by the adoption of the Northumberland mode of sowing that root on dunged drills, it is almost immaterial where the upper stratum is, provided the seed vegetates, as it soon strikes into the manure, and rapidly flourishes.

My chief success, however, has been upon a field of eight acres, which lay in the unprofitable state already described. This land, which is a deep clay, and which had produced a crop of wheat from an old lay sod the former year without any manure, was winter ploughed, and lay in that state until the leveller was introduced the first dry weather in April. It was preceded by two horse ploughs, taking perhaps a square of an acre at once: these loosened the soil the depth of a common furrow, and twice the breadth across the ridges. The leveller followed, drawn by two oxen and two horses, with a man at each handle, to press it down where the height is to be removed, and to lift up the body by the handles where it is to be discharged. Thus four men, one driver, and eight head of cattle, will more effectually level from half an acre to three roods in one day, according as the earth is light or heavy, than sixty or eighty men would accomplish with barrows and shovels, &c., even with the assistance of a plough. In sandy ground, where the depth of one furrow will bring all to a level, as much will of course be done in one day as two ploughs can cover; but my ground required to be gone over several times. After this field was levelled, the backs of the ridges, as they are termed, which were stripped of their vegetable mould, were ploughed up, the furrows not requiring it. They were also harrowed, and the field copiously manured with lime compost; harrowed in, and broke into nine-feet ridges, perfectly straight, in order to introduce Duckit's drill. It was sown under furrow, broad-cast, the last of it not until the 13th of May, and was cut down a reasonable crop

the

the 4th of September. I am now thrashing it, and a sample shall be sent, as well as a return of the eight acres, if necessary.

The field now lies in proper form, well manured, with the advantage of a fair crop from heavy tenacious ground, without losing a season, and in a year by no means favourable.

I am well aware there are many shallow soils where it may be hazardous to remove the enriched surface, and trust perhaps one-half of your land for a crop that had never before been exposed to the atmosphere; but where the soil is sufficiently deep, or you have good under strata, and there is manure at hand to correct what is sour from want of exposure and tillage, it is evident from this experiment that no risk is run.

To avoid the expense of a fallow, and to lay out ground in straight and even ridges, even where drill husbandry is not practised, should be objects to every rational farmer. But where the new system is intended to be adopted, it becomes indispensably necessary. In laying down lawns, parks, &c., where furrows are an eyesore, or places inaccessible to wheel carriages from their declivity, and from which earth is to be removed, it will be found equally useful.

Should the society consider the inventor, David Charles, worthy of any remuneration, honorary or otherwise, it will be gratefully acknowledged by

Your obedient servant,

Westmead,
Jan. 1, 1803.

JOSEPH HARDY.

Mr. Charles Taylor.

Description of the Machine.

Fig. 1. (Plate IX.) A, part of the pole to which the oxen or horses which draw the machine are fastened, and which is attached to the machine by a pin at B.

C C, The two wheels, shod with iron, which run upon the axle D.

E E, The upper frame-work of the machine, extending from the axle to the extremity of the handles F F, and secured firmly by the cross pieces.

G G, The curved iron sliders of the machine, which may be raised or depressed a little by means of the pins H H, which pass through holes in the wood-work, and also in the iron sliders; these sliders form one piece with the back iron scraper I, in the manner more fully explained in fig. 2.

K, The wooden back of the machine, which should be

made strong, to resist the weight of the earth when collected therein. The iron scraper should be firmly secured to this by screws and iron-work.

L L, The wooden sides of the machine firmly connected with the back and frame-work, in order to assist in collecting the earth to be removed.

M, A strong cross piece into which the ribs which support the back are well mortised.

Fig. 2. K, The interior part of the back of the machine.

I, The iron scraper, sharp at the bottom, and firmly screwed to the back of the machine.

G G, Parts of the side irons or sliders, showing the mode in which they are united with the scraper I.

M, The cross piece above described.

LIX. *On the Utility of Public Dispensaries in general; accompanied with a Report of the Cases in the Finsbury and City Dispensaries for the last three Months of the Year* 1805. *Communicated by* John Taunton, *Esq. Surgeon to the City and Finsbury Dispensaries, and Lecturer on Anatomy, Surgery, &c.*

To Mr. Tilloch.

Sir,

There is surely no institution more beneficial or important than that which has for its object the alleviation or cure of disease. Such institutions are the strong-holds of the diseased poor. It is there they seek relief from all their maladies. It is in the arrangements of dispensaries that their circumstances are peculiarly considered.

To these institutions the poor of every denomination have a ready access, and their benefits are extended to every species and degree of disease.

Are their ailments slight or trivial? In the dispensary they find advice and medicine, while their families are not deprived of the earnings of their industry. Are they laid on beds of sickness, or confined by local disease or accidental injury? There are they visited: the dispensary extends its benefits to their habitations. What plan more philanthropic, or more wisely ordered! It is adapted to the circumstances of every malady: it enters into the bosom of every family: it restores the diseased to health, while they enjoy the kind and affectionate offices of the healthful: it extends the benign influence of the healing art to every needy and diseased object.

But

But these are not the only advantages attending such institutions; they also present a wide field for observation and experience to the practitioner. Many cases of rare occurrence come under his care, and he is enabled to collect many useful and important facts.

Thus, while dispensaries extend their salutary influence to the diseased poor, they are subservient to the improvement of the healing art.

Every communication respecting these institutions must be interesting. The manner in which they are supported; the mode in which they are conducted; the aggregate in which they remove or diminish human misery; in which they cure or alleviate disease, and restore the objects of their care to their families, their friends, to the community, are all topics deeply interesting; and not less so are the histories of instructive cases, and the detail of important facts. It is proper that those who contribute to the support of these institutions should know the benefits derived from their philanthropy and munificence: it is proper that others should know them, that they may be induced to follow their example. It is also proper that the history of every instructive case should be faithfully told. The addition of one *real fact* to the general stock of medical or surgical knowledge is a gem of inestimable value.

From these considerations I have resolved to publish the surgical reports of the City and Finsbury Dispensaries, in conducting which I shall observe the following plan:

1st, I shall state the number of patients admitted under my care, in a given time, at each of these institutions.

2dly, I shall state the general events under the heads of cured, relieved, &c. And,

3dly, I shall make a few general observations on some of the more important cases, avoiding, as much as possible, technical terms, or those of art, which could be understood only by persons connected with medical science.

Surgical Patients admitted into the Finsbury Dispensary, St. John's Square, from the 1st of October to the 31st of December, 196.

Cured - - -	111
Relieved - - -	2
Irregular - - -	1
Dead - - -	2
Under cure December 31st -	80
	196

Thirty-

Thirty-one have been home patients, and there have been seven operations; viz. one for an abscess in the tonsal gland, one for a dislocation of the infra maxilla (or lower jaw), one for an abscess in the thigh, one for paracentesis (or tapping for the dropsy), one for a tumour on the forehead, and two for hernia (or rupture).

Mrs. B., St. John's-street, Clerkenwell, had been subject to femoral hernia for several years, which only came down occasionally, was small, and never remained so long as to require surgical aid till the present attack; "though the symptoms, as stated by herself, were violent pain in the abdomen and in the part itself, nausea and vomiting, the tumour receding during the debilitating effects of the sickness."

On Sunday, November 10th, during a violent fit of coughing, the hernia came down, and could not be returned: the part became very painful, from which she suffered much during the night, as well as from the hiccup and almost constant sickness which succeeded. On the 11th, a medical practitioner was called in, whose attempts to reduce the hernia were unsuccessful. He therefore ordered enemas, and gave some cathartic medicines combined with opium. The enemas were immediately returned without producing any effect; and every thing was rejected by the stomach. In the afternoon the symptoms had all increased; the pain extended over the fore part of the abdomen, on which she could not bear the least pressure. A tobacco enema was then given, which produced syncope without any favourable change to the hernia. The operation was then proposed; to which she would by no means consent.

On the 12th she obtained a letter for the Finsbury Dispensary, which was brought to me about four o'clock in the afternoon. On visiting her I made several fruitless attempts to reduce the hernia, which was small but extremely painful. On considering every circumstance, therefore, I concluded that the operation was the best mode of practice that could be adopted. In this opinion I was joined by Dr. Lidderdale, one of the physicians to the dispensary. But she still refused her consent: and some pills were then ordered, in which opium formed a principal ingredient. These could not be retained on the stomach; and the fæculent matter which was now vomited became so distressing, together with the pain, that she consented to have the operation performed; which was done at ten o'clock at night.

The sac in which the hernia was situated was unusually thin,

thin, and did not contain any fluid; so that the greatest care was necessary to avoid wounding the intestine, which was of a very dark colour. The pulse, which varied during the day prior to the operation from 120 to 130, small and thready, was immediately reduced to 86, and was soft and regular. She took some gentle laxative medicines.

Nov. 13th. Has been very easy during the night, but has not had any discharge from the bowels. I ordered an enema, and a gentle dose of Epsom salts; which produced the desired effect: but she complained in the afternoon of being low, and thirsty. Pulse 106.

Nov. 14th. Has had a good night; fever less; pulse 96; but an extremely troublesome cough, for which some expectorant medicines were prescribed.

Nov. 15th and 16th. Continues to recover.

Nov. 17th. Dressed the wound, which had not completely adhered.

Nov. 19th. Recovering fast; appetite good; walks about the chamber, from which she does not experience any inconvenience. From this time the medicines were continued according to circumstances. She was now allowed animal food; the wound healed gradually; and she returned thanks, being perfectly cured, at the beginning of December.

I have been induced to give the above particulars on account of the very quick recovery of the patient from one of the most formidable and frequently fatal operations of surgery; which I attribute principally to the time of operating, the delay of which I have no doubt has proved fatal to many, as the performance of an operation when the vital functions are nearly exhausted can do little more than hurry on the fatal event.

Surgical Patients admitted into the City Dispensary, from the 1*st of October to the* 31*st of December last*, 204.

Cured - - -	84
Relieved - - -	3
Dead - - -	2
Under cure December 31 -	115
	204

Sixty-one of these patients have been visited at home, and ten have undergone operations; two of which, for fistula in ano, of several years standing, have been cured: one for an imperforated anus, which was unsuccessful from the malformation which appeared in the internal canal on examining

mining the body after death, where the colon terminated in a sac, and the rectum was wanting: two cases of cataract treated successfully by couching: one case of an extra thumb removed in a child only a few days old, which healed by the first intention: one case of a tumour that had been growing in the upper lip for four years; it was dissected out by a single incision, which united readily, leaving no appearance of either the operation or disease; the tumour, from its situation and size, had become extremely unsightly: one case of abscess in the hand, where the flexor tendons were laid bare, one of which was necessarily removed: one case of a true scirrhous tumour in the breast, which had been growing for several years, completely discussed by the application of the emp. hydrarg. cum ammoniac., with the saponaceous and camphorated liniments.

Mary Kain, Spitalfields, aged 45, observed a tumour in her right breast about seven years ago, from which she experienced no inconvenience, except occasional darting pain, which did not return frequent, or continue long: it excited very little attention till about May last, when from its size and pain it occasioned much uneasiness, and several professional gentlemen were consulted. At this time the pain, hardness, and irregularity of the tumour appear to have been such as might have truly characterized the disease: however, one of these gentlemen, considering it a common abscess, plunged his lancet into the tumour; but, as will be readily understood, no discharge of matter followed. From this time it ulcerated rapidly, the discharge was thin and offensive, the pain very great, and the constitution suffered so much, that the patient appeared to be going with hasty strides "to that bourne from whence no traveller returns." On the 7th of December she was admitted a patient in the City Dispensary, when the ulcerated surface was nearly of a circular figure of four inches in diameter. She inquired if any operation could be performed; which she was willing to undergo, even at any hazard, rather than endure her present sufferings. The extent of the disease rendered the removal of the whole breast absolutely necessary; which was done on the 9th of December, with little interruption: two arteries only required to be secured till the operation was completed. The pain of the operation gradually left her, so that she had a very comfortable night; and has not from that time experienced either fever or inconvenience in any degree to what might have been expected from an operation of so formidable a nature. No medicines were given, except a few doses of salts to keep the bowels regular, as she

 was

was of a costive habit. The wound has been dressed about every fourth day, and is now nearly healed; her general health is good; and she has been enabled to attend the domestic concerns of her family, without inconvenience, for the last three weeks.

The manner in which this case terminates will be noticed in a subsequent report, as the success of an operation at so late a period in this disease must always be doubtful, and highly interesting.

A cast has been taken of the disease, which is preserved in my anatomical museum.

On this occasion I am extremely happy in having the opportunity of acknowledging the humane attention of Mrs. F****, who, on my representation, kindly visited and administered every necessary comfort to the poor family while the parent was under confinement.

The following Report is extracted from the Minutes of the Committee for managing the Affairs of the City Dispensary, of January 1806.

Surgical Patients admitted into this Institution from the 1st of January to the 31st of December 1805, 917.

Cured - - -	674
Relieved - - -	73
Dead - - -	17
Irregular - - -	7
Not known - - -	2
Under cure December 31 -	144
	917

364 have been visited at home, and 73 have undergone operations.

JOHN TAUNTON.

Greville-street, Hatton-garden,
January 15, 1806.

LX. *Description of M.* BARUEL'S *new Apparatus for making Gaseous Oxide of Carbon. By M.* DRYEUX*.

IT is admitted on all hands, that for the progress that has been made in chemistry within the last thirty years, we are much indebted to the improvements that have been introduced into the apparatus employed. For example, before the apparatus which Woulf invented for obtaining the aëriform fluids disengaged from various bodies when exposed to the action of fire, or presented to other agents susceptible of

* From the *Annales de Chimie*, No. 157.

of uniting with them, chemists were obliged to employ large vessels difficult to be managed, and so inconvenient that it was even impossible to collect those fluids with certainty, the greater part of which were suffered to escape through the opening found necessary in practice to prevent a rupture of the vessels.

At present, with the apparatus of Woulf, we experience none of those inconveniences: the operations carried on during the disengagement of the gas can be made in vessels of very small capacity; we can divide the gases, and determine their quantity and quality very precisely: in fine, such an operation as was formerly very dangerous to the conductor, can now be continued for hours without risk or trouble.

To these advantages many others have been added; and all the world knows that they are owing to that perfection to which chemists have brought the apparatus of Woulf; and, above all, to the happy application which has been made of such apparatuses in a number of circumstances.

But although discoveries of this kind have been carried very far, it is probable that a great number more are yet in reserve; and much praise is due to those who make exertions for this important object, since the apparatuses which they contrive are so many new means put into the hands of chemists to collect an infinity of products, which often escape, and of which the knowledge might have an important effect on the perfecting of chemical science.

From such views, I have thought it would be useful to announce an apparatus invented by M. Baruel, lecturer in the School of Medicine in Paris. This young chemist, perceiving the difficulties and risks that are run in certain processes in which gases are liberated, or when it is necessary to present them for combination to different substances, has endeavoured to obviate the difficulties, and, after several attempts, has produced an apparatus the utility of which has far exceeded his hopes.

I have seen this apparatus employed with great success in the production of gaseous oxide of carbon. This gas, which before could only be obtained with difficulty, and but in small quantities, may now be procured with great facility and with little expense.

The same apparatus may also be used for the preparation of sulphurized hydrogen gas, carbonized and phosphorized hydrogen gas; and may be employed with equal advantage for saturating substances with any gas, especially when the saturation can only be made at a high temperature.

I shall

I shall here give the description of the apparatus as communicated by the author, and exhibit a view and plan of it, which will make its parts more intelligible. (See Plate X.)

If gaseous oxide of carbon is wished to be obtained, let there be first introduced into three gun barrels, B, C, and D, charcoal, very dry and well chosen, broken into small pieces. By means of a thin piece of iron bring the bits of charcoal, but without compressing them, to that part of the gun barrels which is to be exposed to the action of the fire. Place the barrels horizontally in the furnace A, one by the side of the other, leaving between each a distance of about two inches; secure them in their places with clay, and then put on the dome of the furnace. This being done, into one end of the barrel B insert the glass tube E, which is so curved as to allow its other end to be introduced into the neck of the bottle F, which must be large enough to admit also the pipe of the curved funnel G. In the other end of the barrel B is inserted one extremity of the bent tube H; the other end of which tube is fixed into the barrel D, making a communication between D and B. In the same manner, the barrel D is joined to the barrel C by another glass tube I; and lastly, the other end of C receives the tube K, properly bent to pass under the receiver M, placed on the shelf of the pneumatic trough L.

Things being thus disposed, put carbonate of lime mixed with water into the bottle F; lute with care all the joinings of the tubes, and put fire into the furnace. When the gun barrels have acquired a red heat, pour sulphuric acid into the funnel G, which, when it passes into the bottle F, will liberate a quantity of carbonic acid gas from the carbonate of lime. The gas is obliged to pass through the tube E and the barrel B, and by the tube H is conveyed to the barrel D, and thence by the tube I into the barrel C, and through the tube K into the receiver M. The intention of the process is to make the carbonic acid gas pass through among the pieces of ignited charcoal contained in the gun barrels, and thus to saturate itself with as much carbon as it can take up.

It is easy to conceive that this is a more sure and expeditious method than that before employed, in which the gas was made to pass only through a single barrel. It is true the gas was submitted a second, and even a third time, to the same operation; but this was always attended with a waste of time and a loss of gas, which does not take place when this apparatus is employed.

LXI. *Ad-*

LXI. *Additional Experiments and Remarks on an artificial Substance which possesses the principal characteristic Properties of Tannin.* By CHARLES HATCHETT, *Esq. F. R. S.**

§ I.

When I had ascertained that carbonaceous substances, whether vegetable, animal, or mineral, were capable of being converted into a product, which, by its effects on earthy and metallic solutions, on dissolved gelatine, and on skin, resembled the natural vegetable principle called *tannin,* I was at first inclined to give it the name of *artificial* or *factitious tannin*; but some eminent chemists of this country, for whose opinions I have the highest respect, considered this name as objectionable; for, although the artificial substance resembles tannin in the particulars above stated, yet in one character there appears to be a very considerable difference, namely, the effect of nitric acid; for by this the artificial substance is produced, whilst the varieties of natural tannin are destroyed. Such an objection, sanctioned by such authority, induced me to alter the title of my paper, and to expunge the word *tannin* wherever it had been applied to the artificial product.

In order to satisfy myself more fully on this point, I have, since the communication of my former paper, made a few experiments on the comparative effects produced by nitric acid on those substances which contain the most notable quantities of tannin; and of these I shall now give a succinct account, and shall also cursorily notice other experiments in which a tanning substance has been produced, under circumstances different, in some measure, from those which have been already described.

§ II.

Although I cannot as yet assert that the artificial tanning product is absolutely indestructible when repeatedly distilled with different portions of nitric acid, yet the following experiments will prove that the destructibility of it by this method must at least be a work of considerable time and difficulty.

1. Twenty grains of this substance were dissolved in half an ounce of strong nitric acid, the specific gravity of which was 1·40. The solution was then subjected to distillation

* From the *Transactions of the Royal Society* for 1805.

until

until the whole of the acid had come over; after which it was poured back upon the residuum, and the distillation was thus repeated three times.

Care was taken not to over-heat the residuum, and this, when examined, did not appear to have suffered alteration in any of its properties.

2. Ten grains of the artificial tanning substance, mixed with ten grains of white sugar, were dissolved in half an ounce of nitric acid, and the whole was distilled to dryness.

The residuum, being then dissolved in boiling distilled water, and examined by solution of gelatine and other reagents, was found to be unchanged in every respect.

3. This resembled the former, only that gum arabic was employed in the place of sugar. The result was the same.

4. A quantity of dissolved isinglass was precipitated by a solution of the artificial tanning substance, and the precipitate, having been well washed with hot distilled water, was afterwards gradually dried. It was then digested in strong nitric acid, which after some time acted powerfully upon it; much nitrous gas was evolved, and a dark brown solution was formed. This was evaporated to dryness, and, after having been completely dissolved in boiling distilled water, was examined by nitrate of lime, acetite of lead, muriate of tin, and solution of isinglass, all of which formed copious precipitates, similar in every respect to those produced by the artificial tanning substance, which had not been subjected to the above described process.

5. A portion of the precipitate, formed by isinglass and the tanning substance, was dissolved in pure muriatic acid, and was afterwards evaporated to dryness. Boiling distilled water dissolved only a small part, and the solution, which was of a dark beer colour, did not precipitate gelatine, although it acted upon muriate of tin and sulphate of iron; for with the former it produced an ash-coloured precipitate, and with the latter a slight deposit of a reddish brown colour.

6. As so small a part of the precipitated isinglass had been thus rendered soluble in boiling water, the residuum was treated with nitric acid, as in Experiment 4; after which, being evaporated to dryness, it was found to be completely soluble in water, and precipitated gelatine as copiously as at first.

7. I dissolved twenty grains of the pure tanning substance in about half an ounce of muriatic acid; but, after

 distillation

distillation to dryness, the residuum in every respect appeared to be unchanged.

In addition to the above experiments may be added, that the solutions of the artificial tanning substance seem to be completely imputrescible, neither do they ever become mouldy, like the infusions of galls, sumach, catechu, &c.

Having thus ascertained the very unchangeable nature of this substance, I made the following comparative experiments on galls, sumach, Pegu cutch, kascutti, common cutch, and oak bark.

8. Twenty grains of powdered galls were dissolved in half an ounce of the strong nitric acid; the solution was then evaporated to dryness, and the residuum dissolved in boiling water. This did not produce the smallest effect on dissolved gelatine.

9. A strong infusion of galls, evaporated to dryness, and treated as above, was totally deprived of the tanning property.

10. Isinglass precipitated by the infusion of galls was dissolved in nitric acid, and examined as in Experiment 4; but no trace of tannin could be discovered.

11. Twenty grains of sumach were dissolved in half an ounce of the strong nitric acid, and treated as in Experiment 8; after which it appeared that the tannin was destroyed.

12. Twenty grains of Pegu cutch (which contains a considerable quantity of mucilage) were subjected to a similar process, by which much oxalic acid was obtained, but every vestige of tannin was obliterated.

13. Twenty grains of the catechu called *kascutti* afforded results similar to the above.

14. Twenty grains of the common cutch or catechu, being dissolved in nitric acid, evaporated to dryness, dissolved in water, and examined by solution of isinglass, rendered the latter turbid: a tenacious film was deposited, which was insoluble in boiling water, and was evidently composed of gelatine and tannin.

15. Twenty grains of prepared oak bark, by the like treatment, afforded a solution in water, which still acted in some measure upon gelatine, as it caused a solution of isinglass to become slightly turbid; and a film, completely insoluble in boiling water, was, as in Experiment 14, deposited on the sides and bottom of the vessel.

16. Infusions were prepared, as nearly as possible of equal strength, from galls, sumach, shavings of oak wood, oak bark, and the artificial tanning substance: half an ounce, in measure, of each was then put into separate glasses,

glasses, and one drachm, in measure, of the strong nitric acid was added.

The different infusions were then examined by solution of isinglass, and I found that those of galls, sumach, and oak wood, were not rendered turbid, whilst the contrary happened to the infusions of oak bark, and of the artificial substance; for these continued to precipitate gelatine, until four drachms, or half an ounce, of the nitric acid had been added to each half ounce of the infusion.

When the results of these experiments are compared, they seem to establish, that although the artificial product is by much the most indestructible of all the tanning substances, yet there is some difference in this respect even between the varieties of natural tannin; and that common catechu, and the tannin of oak bark, resist the effects of nitric acid much longer than galls, sumach, kascutti, and Pegu cutch. The last, as I have observed, is replete with mucilage, and by nitric acid yields a large quantity of oxalic acid; it also appears to be the most destructible of all the varieties of catechu; and on this account I attempted, although without success, to promote the destruction of the properties of the artificial substance by adding gum arabic in one case, and sugar in another, to different portions, previous to exposing it to the action of nitric acid. I am, however, convinced that the presence of gum or mucilage in natural substances which contain tannin, renders this more speedily destructible by nitric acid; and I shall soon have occasion to notice some experiments which tend to prove that the presence of gum or mucilage in certain bodies, also prevents, or impedes more or less, the formation of the artificial tanning substance. The cause of this difference, I am inclined to suspect, is, that in those bodies the gum or mucilage is not simply mixed, but is present in a state of chemical combination, by which certain modifications produced by the action of nitric acid upon the elementary principles of the original substance become facilitated.

§ III.

A. When sulphuric acid was added to a solution of the artificial tanning substance, the latter became turbid, and a copious brown precipitate subsided, which was soluble in boiling distilled water, and then was capable of precipitating gelatine.

B. The same effect was produced by muriatic acid; so that in these particulars the artificial tanning substance was

 found

found to resemble precisely the tannin of galls and of other natural substances*.

C. Carbonate of potash, when added to a solution of the artificial tanning substance, deepens the colour; after which the solution becomes turbid and deposits a brown magma.

D. Five grains of the dry substance were dissolved in half an ounce of strong ammonia; the whole was then evaporated to dryness, and, being dissolved in water, was found not to precipitate gelatine, unless a small portion of muriatic acid was previously added.

E. Another portion of the same substance, which had been dissolved in ammonia, was evaporated in a long-necked matrass, and was kept in very hot sand during half an hour; at first some ammonia arose, and afterwards a yellow liquor which had the odour of burned horn. The residuum was then examined, and was found to be nearly insoluble in water, to which it only communicated a slight yellow tinge.

F. It is remarkable that the dry artificial tanning substance, although prepared from vegetable matter, should, when placed on a hot iron, emit an odour very analogous to burned animal substances, such as horn, feathers, &c.; this I found also to be the case in the experiment which has been related, and I was desirous, therefore, to ascertain more accurately the effects of heat on this substance when distilled in close vessels.

I took some very pure vegetable charcoal which had been exposed to a red heat in a retort for more than an hour, and by nitric acid converted it into the artificial tanning substance.

Twenty grains of this, rendered as dry as possible, were put into a small glass retort, to which a proper apparatus, terminating in a jar filled with quicksilver, and inverted in a mercurial trough, was adapted. The retort was placed in a small furnace, and was gradually heated by a charcoal fire until the bulb became red hot.

When the retort became warm, and after the expulsion of the atmospheric air, a very small portion of water arose, which settled like dew on the sides of the vessels: this was succeeded by a little nitric acid, from which the tanning substance had not been completely freed; and soon after a yellowish liquor came over, which was in so very small a quantity as only to stain the upper part of the neck

* Mr. Davy on the Constituent Parts of Astringent Vegetables. Phil. Trans. 1803, p. 240, 241.

of

of the retort: as nothing more seemed to be produced, I then raised the fire, when suddenly the vessels were filled with a white cloud, and so great a portion of gas was almost explosively produced, as to overset the jar: this gas, by its odour, appeared to be ammonia, which in the first instance had formed the white cloud, by combining with the vapour of the nitric acid, with which the vessels were previously filled *. Another jar was speedily placed in the room of that which had been overturned, and a quantity of gas was slowly collected: this proved to be carbonic acid, excepting a very small part, which was not taken up by solution of caustic potash, and which, as far as the smallness of the quantity would permit to be determined, appeared to be nitrogen gas. There remained in the retort a very bulky coal, which weighed eight grains and a half: this, by incineration, yielded one grain and a half of brownish white ashes, which consisted principally of lime; but whether any alkali was also present I cannot positively assert, as the trace which I thought I discovered of it was very slight.

I shall for the present postpone any remarks upon this experiment, as I wish to proceed in the account of others which have been made on the artificial tanning substance.

G. Fifty grains of this substance were dissolved in four ounces of water, and were afterwards precipitated by dissolved isinglass, eighty-one grains of which became thus combined with forty-six grains of the tanning substance.

The remaining portion of the latter was not precipitated, and was therefore separated by a filter, and evaporated to dryness. It then appeared in the state of a light brittle substance of a pale cinnamon brown colour; and it is very singular, that although charcoal is an inodorous body, and although the artificial tanning substance, when properly prepared, is likewise devoid of smell, (unless a certain pungent sensation which may be perceived upon first opening a bottle containing the powder after agitation should be so termed, but which seems rather to be a mechanical effect,) yet this substance possessed a strong odour not very unlike prepared oak bark; and this odour became much more perceptible when the substance was put into water; in which it immediately dissolved. The solution was extremely bitter, and acted but slightly on dissolved isinglass, with which, however, it formed some flocculi; with sulphate of iron it produced a brown precipitate; with muriate of tin,

* After the experiment the receiver was found to be thinly coated with a white saline crust.

one which was blackish brown; nitrate of lime had not any effect, but acetite of lead occasioned a very copious precipitate of a pale brown colour. This substance, therefore, appeared to be a portion of the tanning matter so modified as partly to possess the characters of extract *.

Other experiments were made on the tanning substance prepared from various bodies, which by the dry and by the humid way had been previously reduced to the state of coal: but these I shall here omit, and shall pass to the description of a series of experiments, by which I obtained a variety of the artificial tanning substance in a way different from that which has been related, and with which I was unacquainted when my former paper was written.

[To be continued.]

LXII. *Extract from a Memoir by Messrs.* FOURCROY *and* VAUQUELIN *upon the Phænomena and the Products which Animal Matters afford when treated with Nitric Acid. Read at the National Institute* †.

THE discovery of azote in animal substances, by the labours of M. Berthollet, and the disengagement of this principle during the application of the nitric acid to these substances, is one of the most beautiful improvements in modern chemistry.

Messrs. Fourcroy and Vauquelin, by repeating these experiments on muscular flesh, have added to this valuable fact others of a very interesting nature.

We shall proceed to give a concise account of their experiments, and of the results which they have yielded.

I. A mixture of 150 grammes of muscular flesh, with an equal quantity of nitric acid of 32 degrees strength, and of water, when introduced into a matrass and heated to gentle ebullition, gave out 96 inches of gas, which was found to consist of nine-tenths azote and one-tenth carbonic acid.

The residuum consisted of the remains of the flesh, still retaining, in part, its fibrous appearance; of a yellow liquor; and of a layer of yellow fatty matter on the surface of the liquor. Having separated this matter and filtered the liquid,

* When added to a solution of carbonate of ammonia it produced some effervescence, but its peculiar vegetable odour did not suffer any diminution.

† From the *Annales de Chimie*, No. 166.

quid,—they submitted the fibrous residue to the following trials:

Boiling water was rendered yellow by it, and acquired at the same time the property of converting vegetable blues to a red; even the last portions of water assumed a yellow colour, although they no longer showed the presence of any acid. The residuum, after washing, acquired a still deeper colour, and when mixed with a little water reddened again the paper of turnsole.

When dissolved in alkalies it had a deep blood red colour: acids, on the other hand, precipitated it in the form of yellow flakes.

This matter is greasy and pitchy to the touch; it smells like rancid fat, and has a very acrid taste.

The fusion and swelling which it undergoes when placed over burning coals, the fumes of grease and the fœtid odour which it emits during this process, and the small portion of charcoal which it leaves, render it akin to fatty matters, although in fact it appears to be acid.

II. A more minute examination of this yellow substance has shown it to possess the following properties:

It neutralizes the alkalies so as to alter their qualities almost entirely: its combinations with potash and ammonia froth like a solution of soap: they are not decomposed by the carbonic acid; and they precipitate the solutions of mercury and lead in yellowish white flakes.

The yellow substance decomposes the alkaline carbonates with effervescence even in the cold, and the solution of acetate of potash with the assistance of a gentle heat.

On the application of alcohol, to which the celebrated authors of the memoir had next recourse, the substance in question is found to consist of a small quantity of greasy matter which the alcohol takes up, and of an acid, to which on account of its colour they have given the name of the *yellow acid.* When deprived of the grease which alters its properties, the yellow acid assumes a deeper colour: it reddens in a greater degree turnsole paper; it no longer melts in the same manner, and exhales not the odour of burnt grease, but fœtid and ammoniacal vapours.

The yellow acid dissolves in grease, and renders it at the same time acrid and rancid. It combines with ammonia, which it deprives of its odour; it yields, by distillation, all the products of animal substances; therefore consists of azote, hydrogen, carbon, and oxygen; and ought to be ranked among the animal acids.

III. The yellow matter, composed of the yellow acid

 and

and grease, when subjected anew to the action of nitric acid at the temperature of about 50°, underwent no very remarkable change. Its yellow colour was converted to a whitish; its specific gravity and volume were diminished, but without any motion or effervescence in the acid; it reddened blues very strongly; it dissolved, as before, in a solution of potash, to which it communicated an orange red colour; and its taste was extremely acrid. The action of the nitric acid upon the yellow substance extends no further than reducing it nearly to the state of an oil, and does not destroy the acid properties which were originally communicated to it.

IV. The nitric acid which had been employed in the decomposition of the muscular flesh was next examined. Its yellow colour resembled much the colour of a solution of chromate of potash. During the supersaturation of the liquor with carbonate of potash it first assumed an orange colour, then became muddy, and let fall a small quantity of an orange red powder. When this mixture was subjected to distillation it gave over a clear liquor, without colour, of a rancid greasy smell, and containing a small portion of ammonia, probably formed by the action of the nitric acid. What remained in the retort had assumed a dark brown colour; but it was neglected to be examined.

Another portion of nitric acid which had been employed in the same way yielded on distillation a colourless liquor, having the same taste and odour. The liquid remaining in the retort assumed a deeper yellow when concentrated, and its re-action on the nitric acid became speedily apparent by the copious disengagement of red vapours; when reduced to forty grammes it crystallized in flattened needles, in the midst of a mother water which was thick and tenacious like a solution of gum.

The mother water was of an acid and bitter taste, assumed a blood red colour on the addition of a small quantity of pure potash, and when mixed with alcohol let fall a white flocculent matter which formed into fine semi-transparent grains, having an agreeably acid taste. Five decigrammes of this salt left after calcination twenty-one centigrammes of a yellowish residuum, which was very light, and dissolved with effervescence in nitric acid: the solution yielded on evaporation needles of sulphate of lime and nitrate of potash. This saline precipitate, thrown down by alcohol, was found to consist of a mixture of sulphate of lime and acidulous oxalate of potash.

The mother water already treated with alcohol yielded,

on the addition of lime water, a second precipitate, which was found to be no other than the oxalate of lime. By gently evaporating the fluid which remained after these two precipitations the liquor thickened into a viscid brown syrup, having a bitter taste somewhat similar to that of walnut rind: when mixed with a large quantity of alcohol a very abundant separation of a white matter took place, and the whole was formed into a coagulum. This matter consisted of very pure malate of lime, and the alcohol containing in solution the yellow bitter substance.

The learned authors of the memoir from which we give a detailed extract, infer from the facts above stated, 1. That muscles contain potash, lime, and sulphuric acid, or rather sulphur which has been oxygenated by the action of the nitric acid: 2. That a portion of the muscular fibre, or rather of the cellular substance in which it is enveloped, becomes converted by the nitric acid into malic and oxalic acid.

The alcohol employed to effect the separation of the malate of lime held in solution, 1. a small quantity of nitrate of lime; 2. a reddish brown matter, having a very bitter taste, with a flavour like that of walnut, of which we shall speak hereafter; 3. a small portion of the detonating substance which has been found in indigo: it was obtained in this instance by the concentration of the alcoholic solution, and separated in the form of granulated crystals, which were highly inflammable, and detonating, upon the addition of carbonate of potash.

V. A little consideration of this analysis will readily convince us of the importance of the inferences which it affords, more especially when we compare the information hitherto possessed upon this subject with the more extended ideas which it opens upon a point so interesting in its consequences and in its application to the animal œconomy: indeed we may almost venture to assert, that there is now little left to be desired upon the subject.

The disengagement of azote, and the formation of carbonic acid, of fat, of oxalic acid, and of a bitter matter, were all the facts formerly known regarding the action of nitric acid upon animal substances: to these our authors have added the discovery, 1. of a yellow and almost insipid matter, which, although acid, dissolves with difficulty, and which immediately replaces the fleshy fibre; 2. of another yellow substance, bitter to the taste, more soluble, and equally acid, which remains dissolved in the nitric liquor; 3. of an inflammable detonating substance which

which also remains in solution; 4. of the formation of the malic acid.

It appears, and it is the opinion of Messrs. Fourcroy and Vauquelin, that the yellow and little soluble matter is the first degree of change which the muscular fibre undergoes; it passes immediately into a second stage of alteration and acidity, and the yellow soluble matter is produced: this, by a third change, is replaced by the inflammable and detonating substance forming the third and last step in the decomposing action of the nitric acid. They attribute the successive formation of these three compounds to the subtraction of a portion of the azote, and of a larger quantity of the hydrogen of the muscular flesh: in this way the relative proportions of their elements are altered, and there remains an excess of carbon and oxygen, which communicates those characters of fat and acidity which we remark. The proportion of the principles constituting these three compounds is a question of such difficulty that we cannot expect it will soon be resolved.

They are convinced that the acidity of the yellow substances depends in no degree upon the presence of nitric acid, as they have failed in every attempt to detect it.

The oxalic and malic acids, they conceive, are formed from the white and mucous layers of cellular substance; and the opinion is confirmed by comparative experiments on the white and membranous parts of the body, which show that these yield, on the application of the nitric acid, a large quantity of the malic and oxalic, but very little fat or yellow matter.

VI. Some insulated facts, which have hitherto appeared susceptible of no useful application, seem naturally to connect themselves with those presented by the analysis before us; and the learned chemists to whom we owe it have very happily pointed out the connection. Such are the facts which were offered by the examination of the biliary concretions of some animals, more especially those found in the gall bladder of the ox: such also are the resemblances which serve to show the connection between the bile and the colour of the skin and urine in cases of *icterus* (jaundice), and the yellow substances which are mentioned in this memoir.

Further experiments, instituted with a view to verify their suspicions, were attended with the most favourable results. The red matter of biliary concretions, when separated by alcohol from the green bitter matter with which it is mixed, presented the same characters which distinguished the first

yellow

yellow matter furnished by the action of nitric acid upon muscular flesh.

They obtained from the urine of a young man labouring under slight jaundice, a red substance, having an almost exact resemblance to the matter afforded by muscle and nitric acid. It was procured by treating with alcohol a quantity of urine which had been evaporated to the consistence of honey. The alcohol contained the red matter which they sought, besides a large quantity of urea, of sal-ammoniac and of acetate of soda, which the patient had been using internally.

These experiments, executed with that admirable talent and ingenuity which the celebrated authors of this memoir manifest in all their inquiries, would induce us to conceive, with them, that jaundice is produced by the superabundant quantity of this matter, and its consequent introduction into the system of the cutaneous absorbents: that it is this substance which communicates a yellow colour to the bile and to the biliary calculi, which afford, on analysis, evidence of its presence: that this yellow acid is formed in the animal œconomy either by the oxygenation of the muscular fibre, or of the fibrine of the blood.

Does there not appear to be a decided similarity between this yellow acid matter and the acid which is found in fat that has been long exposed to the air, as well as in that which has acquired a yellow colour from disease, and in the fat treated with nitric acid so as to form oxygenated ointments?

Are we not forced to acknowledge that these ideas acquire great additional probability from considering that the acetate of soda, the alkaline carbonates, the yolks of eggs, are at once the remedies which are most successful in the treatment of jaundice, and the best chemical solvents of the yellow acid, or of that fatty and acid matter which so evidently is present in *icterus*?

And lastly, Can we still remain in the opinion that the expectation of determining the proximate cause of diseases is founded on a mere chimera; and that the discoveries of chemistry, or successful researches into animal substances, will fail to throw the most important light upon the nature of diseases, and the mode of their cure?

LXIII. *A Me-*

LXIII. *A Memoir on the Means of rendering Smutty Wheat fit for Market. Translated from the Bibliotheque Physico-Economique by the Rev.* JOHN DUBOURDIEU *.

THIS memoir has two objects; the first, to increase the value of smutty wheat in the market; the second, to prepare bread of a good quality from it.

All wheat is called smutty when the chests of some of the ears contain a black dust instead of a farinaceous substance, which the flail breaks in the operation of threshing, and which adhering to the sound grain spoils its colour, and communicates a degree of blackness to the flour.

Disadvantages of Smutty Wheat.

The disadvantages are in proportion to the quantity of diseased grain, and are as follow:—The infected grain never sweats † completely in the granary, and consequently it cannot be preserved so long; for the black dust with which it is covered prevents the moisture from escaping in the granary. If it is sent to market, it is sold four francs or a hundred sols by the *setier* ‡ cheaper than grain of the same quality which is not diseased. When it is taken to the mill, even after having remained a considerable time in the granary, it clogs the mill-stones, greases the bolting-sieves, delays the grinding, and gives less flour. The flour of good grain, ground after that which is smutty, is contaminated by the remains of the latter. The flour of smutty grain is of a dirty white, soft and greasy to the touch; in baking, it proportionably absorbs but little water, emits a smell like rancid grease, and is difficult to keep. After this the author mentions the different modes that have been tried to remedy this defect in grain, and which have proved ineffectual: 1st, that of sifting it often; 2dly, drying it, and turning it in the granary; 3dly, by mixing an absorbent earth reduced to powder with it; 4thly, by sprinkling it with water, drying, and sifting it. He then mentions washing the grain completely as the only method of freeing it from the effects of this disease, which those farmers in France that were anxious to bring only grain of the first quality to market have hit upon.

* From *Transactions of the Dublin Society*, vol. iii.

† *Ressue* is the word I have translated *sweats*.

‡ A French measure equal to twelve bushels.

Mode

Mode of washing Smutty Grain.

Water from wells, springs, or streams, may be used with equal effect: to employ it, proper vessels must be procured, as buckets, shallow tubs, and casks open at one end; but in preference to these, a large flat cistern with a sluice, the inside of which should be covered with a piece of linen, or of tin perforated, to facilitate the escape of the water impregnated with the smutty matter, and to prevent the passage of the grain. This operation is more easily performed when near the water: in that case the wheat may be washed in baskets so close as to retain the grain. The mere motion of the water is insufficient to detach the dust from the wheat; it must be stirred with a broom, and rubbed with the hands, in small quantities at a time; the foul water must be let out of the cistern, and fresh water put upon the wheat, until it runs off clear. If it is washed at a river or a well, the basket must be plunged in it several times. It is, however, to be observed, that this operation must be performed as quickly as possible, that the grain may be washed without being softened, to prevent the difficulty in drying it, and to avoid wrinkling the skin.

Drying the washed Grain.

The moment the grain is taken out of the water, it ought to be spread on cloths in the open air; in the southern provinces, where it is customary to wash grain, it is dried in the sun. This method is preferable to any other, and at the same time the most œconomical; and in our climate (France) a cogent reason for washing smutty grain immediately after harvest, is to take advantage of the heat of the sun in the latter end of August, or the beginning of September: besides, the sooner the grain is washed, the more easy and effectual is the operation.

If the weather will not permit the grain to be dried in the open air, it must be spread very thin on the floor of the granary, and often turned to prevent its heating, and to allow a free escape to the adventitious moisture. If the weather happens to be moist and warm, and likely to make it sprout, it should be dried in an oven moderately heated. In whatsoever way the grain has been dried, care must be taken not to leave it in heaps, nor to press it closely together, until it is perfectly cool, and has been passed two or three times through the riddle.

Of

Of the Advantages of washing and drying Grain.

These operations are not difficult; they only require time; and the farmer who performs them will not have occasion to regret the moments thus employed when he reflects on the consequent advantages. These advantages are, the power of preserving his grain, or of using it immediately, or of mixing it with old grain, without any inconvenience. It restores to wheat its natural facility in grinding; it makes the produce in flour greater than when unwashed; and the flour has not that dirty whiteness, and disagreeable smell, attendant on smutty grain; it is more easy to keep, and it absorbs more water in baking. Bread made from this wheat has all the qualities that can be wished for in this aliment. If the grain is to be sold, it acquires at market a value of one-fourth above that which is smutty. But it may be objected, that by washing wheat it loses the quality of *handling well*, and from this circumstance suffers a diminution in value. On this it must be remarked, that if the operation is quickly executed, particularly if the grain is dry, the skin will not be penetrated by the moisture; that it will not be wrinkled, and will preserve its size and smoothness. Its loss of weight will be small when compared with the advantages above stated.

In all circumstances which have required it, this practice has been followed with success both by great and small farmers, by millers who deal in wheat, and by private persons who manufacture their own flour. It is the constant mode pursued by the best bakers, who otherwise could not make bread of the first quality in those seasons when smutty wheat is common, and in such years has been the means of enriching those active and intelligent persons who have had recourse to it.

Reflections.

After what has been mentioned in this memoir, the farmer may be convinced that nothing but washing and drying can remedy the disadvantages of smutty wheat. It is, then, his interest to perform this operation, because, if he does it not, the miller or the baker will do it for him, and have all the advantage which he might obtain for himself. Even for his own consumption he ought to practise it, as both himself and his family will eat better bread, which costs less in baking: in a word, he ought to employ this method, as well for his own credit as for the sake of humanity, because flour made from wheat thus prepared cannot be unwhole-

some;

some; and thus the farmer will be free from all uneasiness on this subject, and from every well founded reproach.

Observation by the Translator.

The climate in France being so much warmer than ours, is the reason the author mentions ovens for the purpose of drying wheat: in close and moist weather, after washing, the sun and air being in general sufficient to prepare wheat for grinding, kilns are not in use as with us, where all grain, unless kept a considerable time on the straw, requires that process to make it *work*; nor will it keep in any considerable quantity unless kiln-dried, and even in that state it requires constant turning: therefore to millers, who always dry their wheat, and who of course are near to water, the operation of washing seems peculiarly adapted in seasons when the smut is common.

The author does not seem to be acquainted with lashing of wheat to free it from the straw: when this operation is performed, the smut balls are not broken as they are by the flail, and consequently less of the black powder adheres to the grain; and the balls, being lighter than the wheat, may be separated by repeated and careful winnowing. But the second quality, which is obtained by threshing, after lashing, and amongst which the smut balls are broken, ought certainly to be washed.

LXIV. *Specification of the Patent granted to* Arthur Woolf, *of Spa Fields, in the County of Middlesex, for certain Improvements in the Construction of Steam-Engines: Dated June* 7, 1804*.

To all to whom these presents shall come, &c. Now know ye, that in compliance with the said proviso, I the said Arthur Woolf do hereby describe and ascertain the nature of my said invention, and the manner in which the same is to be performed, as follows: that is to say, I have ascertained, by actual experiments, and have applied the same to practice, that as in practice it is found that steam acting with

* In our nineteenth volume, p. 133, we gave some account of the subject of this patent; and in our present volume, p. 123, we laid before our readers Mr. Woolf's specification of a subsequent patent for improvements on steam-engines, which may be considered as a supplement to the former. A number of our readers having, however, requested that an entire copy of Mr. Woolf's specification of his first patent might appear in our work,—in compliance with their wishes we now insert it.

with the expansive force of four pounds the square inch, against a safety-valve exposed to the atmosphere, is capable of expanding itself to four times the volume it then occupies, and still to be equal to the pressure of the atmosphere; so in like manner steam of the force of five pounds the square inch can expand itself to five times its volume. Masses or quantities of steam of the like expansive force of six, seven, eight, nine, or ten pounds the square inch, can expand itself to six, seven, eight, nine, or ten times its volume, and still be respectively equal to the atmosphere, or capable of producing sufficient action against the piston of a steam-engine, to cause the same to rise in the old engine (with a counterpoise) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines first brought into effect by Boulton and Watt: and this ratio is progressive, and nearly, if not entirely, uniform; so that steam of the expansive force of twenty, thirty, forty, or fifty pounds the square inch of a common safety-valve, will expand itself to twenty, thirty, forty, or fifty times its volume; and, generally, as to all the intermediate or higher degrees of elastic force, the number of times which steam of any temperature and force can expand itself is nearly the same as the number of pounds it is able to sustain on a square inch, exposed to the common atmospheric pressure; provided always, that the space, place or vessel, in which it is allowed to expand itself, be at least of as high a temperature, or nearly as high a temperature, as that of the steam before it be allowed room to expand: that is, whatever be the degree of heat necessary to the permanency of steam of the force of twenty pounds the square inch, if steam of that force be employed, the space, place, or vessel in which it is allowed or intended to expand itself, should be of the same temperature, or nearly so; and so with steam of any other power, as of thirty, forty, or fifty pounds the square inch, the space, place, or vessel in which it is to expand, should be at or about the same degree of heat as steam of the force employed requires for its existence; in which case, as I have before stated, steam can expand itself about as many times as the number of pounds it could have sustained on a square inch as aforesaid, before it is allowed to expand or dilate itself. Here, however, it may be necessary to remark, that in stating this ratio I only speak of the expansion of steam as it can be managed and commanded in practice, and not of the absolute expansion which perhaps might be obtained if mechanism could be made so perfect as to prevent all escape of steam, and all partial condensation

densation of it and waste of heat; for the real expansive force of steam, I am inclined to believe, from the experiments I have made, increases in a regular ratio a little beyond what I have stated, though I would not recommend that it should be calculated higher in applying it to steam-engines, because the difficulty of confining and managing it increases also as the elasticity of the steam is increased, or as its temperature is increased.

And here it may be of use to the public to state some facts respecting different degrees of temperature required to bring steam to, and maintain it at, different expansive forces above the weight of the atmosphere; because the temperature of the steam indicated by a thermometer, having its bulb in the boiler which produces it, will indicate the expansive force of the steam, without the trouble and inconvenience of changing the weights on the valve, by which its force is regulated for the work intended to be performed by it, and which valve acts as a common safety-valve, so that those who attend the boiler will know with sufficient precision, by looking at the thermometer, how they ought to feed the fire; and, moreover, the relation between the temperature and the expansive force being known, the danger of accidents from the safety-valve becoming deranged will be lessened, for the workman will naturally be led to notice whether the safety-valve acts freely when the thermometer has risen to the degree that answers to the weight with which the valve is loaded for working. I have found by actual experiment, setting out from the boiling point, or two hundred and twelve degrees of the thermometer, commonly employed in this country, which is that of Fahrenheit, at which degree steam of water is only equal to the pressure of the atmosphere, that, in order to give it an increased elastic force equal to five pounds the square inch, the temperature must be raised fifteen or sixteen degrees, or to about two hundred and twenty-seven and a half, when the steam will have acquired a power to expand itself to five times its volume, and still be equal to the atmosphere, and capable of being applied as such in the working of steam-engines according to my said invention. And with regard to various other pressures, temperatures, and expansive forces of steam, the same are shown in the following table:

Table of the relative pressures per square inch, temperatures and expansibility of steam at degrees of heat above the boiling point of water, beginning with the temperature of steam of an elastic force equal to five pounds per square inch, and extending to steam able to sustain forty pounds on the square inch.

	Pounds per square Inch.		Degrees of Heat.		Expansibility.	
Steam of an elastic force predominating over the pressure of the atmosphere upon a safety-valve,	5	requires to be maintained by a temperature equal to about	227½	and at these respective degrees of heat, steam can expand itself to about	5	times its volume, and continue equal in elasticity to the pressure of the atmosphere.
	6		230¼		6	
	7		232¾		7	
	8		235¼		8	
	9		237½		9	
	10		239½		10	
	15		250½		15	
	20		259½		20	
	25		267		25	
	30		273		30	
	35		278		35	
	40		282		40	

And so in like manner, by small additions of temperature, an expansive power may be given to steam to enable it to expand to fifty, sixty, seventy, eighty, ninety, one hundred, two hundred, three hundred, or more times its volume, without any limitation but what is imposed by the frangible nature of every material of which boilers or other parts of steam-engines have been or can be made; and prudence dictates that the expansive force should never be carried to the utmost the materials can bear, but rather be kept considerably within that limit.

Having thus fully explained my discovery of the expansive power and force of steam, I shall proceed to describe my improvements grounded thereon; and in so doing, I shall find it necessary to mention the entire steam-engine, and its parts, to which, as an invention well known, I neither can nor do assert any exclusive claim; but at the same time I must here observe, that, from the nature of my said discovery, and its application, there can be no difficulty in distinguishing my said improvements from the improved engine, as to its other common and well known component parts.

1st. If the engine be constructed originally with the intention of adopting my said improvements, it ought to have two steam-vessels of different dimensions, according to the temperature or the expansive force determined to be communicated to the steam made use of in working the engine; for the smaller steam-vessel, or cylinder, must be a measure

measure for the larger. For example, if steam of forty pounds the square inch is fixed on, then the smaller steam-vessel should be at least one-fortieth part the contents of the larger one. Each steam-vessel should be furnished with a piston, and the smaller cylinder should have a communication both at its top and bottom (top and bottom being here employed merely as relative terms, for the cylinders may be worked in a horizontal, or any other required position, as well as vertical). The small cylinder, I say, should have a communication both at its top and bottom with the boiler which supplies the steam; which communications, by means of cocks or valves of any construction adapted to the use, are to be alternately opened and shut during the working of the engine. The top of the small cylinder should have a communication with the bottom of the larger cylinder, and the bottom of the smaller one with the top of the larger, with proper means to open and shut these alternately by cocks, valves, or any other well known contrivance. And both the top and bottom of the larger cylinder or steam-vessel should, while the engine is at work, communicate alternately with a condensing-vessel, into which a jet of water is admitted to hasten the condensation; or the condensing-vessel may be cooled by any other means calculated to produce that effect. Things being thus arranged, when the engine is at work, steam of high temperature is admitted from the boiler, to act by its elastic force on one side of the smaller piston, while the steam which had last moved it has a communication with the larger steam-vessel or cylinder where it follows the larger piston now moving towards that end of its cylinder which is open to the condensing-vessel. Let both pistons end their stroke at one time, and let us now suppose them both at the top of their respective cylinders, ready to descend; then the steam of forty pounds the square inch entering above the smaller piston will carry it downwards, while the steam below it, instead of being allowed to escape into the atmosphere, or applied to any other purpose, will pass into the larger cylinder above its piston, which will take its downward stroke at the same time that the piston of the smaller cylinder is doing the same thing; and while this goes on, the steam which last filled the larger cylinder in the upward stroke of the engine will be passed into the condenser, to be condensed during the downward stroke. When the pistons in the smaller and larger cylinder have thus been made to descend to the bottom of their respective cylinders, then the steam from the boiler is to be shut off

 from

from the top, and admitted to the bottom of the smaller cylinder, and the communication between the bottom of the smaller and the top of the larger cylinder is also to be cut off, and the communication to be opened between the top of the smaller and the bottom of the larger cylinder; the steam, which in the downward stroke of the engine filled the larger cylinder, being now open to the condenser, and the communication between the bottom of the larger cylinder and the condenser shut off; and so alternately admitting the steam to the different sides of the smaller piston, while the steam last admitted into the smaller cylinder passes alternately to the different sides of the larger piston in the larger cylinder, the top and bottom of which are made to communicate alternately with the condenser. In an engine working with the improvements which have been just described, while the steam is admitted to one side of the piston in the smaller cylinder, the steam on the other side has room made for its admission into the larger cylinder on one side of its piston, by the condensation going on on the other side of the large piston which is open to the condenser; and that waste of steam which takes place in engines worked only by the expansive force of steam, from steam passing the piston, is prevented; for all steam that passes the piston in the smaller cylinder is received into the larger. In such an engine, where it may be more convenient for any particular purpose, the arrangement may be altered, and the top of the smaller made to communicate with the top of the larger, and the bottom of the smaller with the bottom of the larger cylinder; in which case the only difference will be, that when the piston in the smaller cylinder descends that in the larger will ascend, and while the latter descends the former will ascend; which for some particular purposes may be more convenient than the arrangement before described.

2dly. As the difficulty of giving a proper degree of strength to large cylinders, and the cases for the same, which are to be exposed to the action of strong steam, increases in proportion as their size is augmented; instead of employing, besides the smaller cylinder or steam measure, one cylinder only of large diameter in steam-engines of great power, I sometimes prefer substituting for the latter cylinder two or more cylinders of smaller diameter, but of such dimensions that their capacity, and the area of the pistons worked in them, are equal to the area of the one piston, and the capacity of the one cylinder which otherwise would be necessary; and such substituted cylinders are made to communicate

municate with each other, that they may at one and the same time receive and part with the steam by which the engine is worked; and their respective piston rods are so connected with one another, or with other parts of the machinery, that the pistons may act together.

3dly. With regard to steam-engines which are already constructed, I improve the same by adding thereto a small cylinder or measure for the steam admitted to the working cylinder: this improvement may be introduced with great advantage into the steam-engines constructed according to Mr. Watt's improvements; which small cylinder may be either employed only as a measure of the steam, or a piston may be worked in it by the expansive force of the steam, before it be allowed to pass into the present working cylinder or steam-vessel, which in that case must have no direct communication with the boiler, but must be supplied with steam through the medium of the small added cylinder or steam measure. To make this more intelligible, suppose the steam measure or small cylinder added to one of Mr. Watt's single engines, or to one of similar construction, and that the small added cylinder is furnished with a piston to work by the expansive force of the steam, while the larger cylinder works by condensation; then all that is necessary is, that the top of the smaller added cylinder be connected with the boiler, and furnished with a cock or valve to shut off the steam, and that the boiler be of sufficient strength: let a pipe pass from the top to the bottom of the smaller added cylinder, furnished with a cock or valve, by which a communication may be made between the two sides of the piston in the said cylinder; let a pipe also pass from the bottom of the smaller cylinder to the top of the larger; let the larger cylinder, as is now commonly the case in practice, have a pipe passing from its top to its bottom, by which a communication can be made between the two sides of the piston in the large cylinder; and let the large cylinder communicate with a condensing-vessel, with valves, cocks, or any other contrivance to open or shut the different communications when necessary. Things being thus arranged, suppose the engine at work, and the pistons in both cylinders at the top of their respective cylinders, and suppose the steam admitted to enter the small cylinder above the piston: while the steam is thus entering, let the communication between the bottom of the small cylinder and the top of the large one be open, while the bottom of the large one is open to the condenser; then both pistons will descend. When they have reached the

 bottom

bottom of their respective cylinders, the communications between the boiler and the small cylinder, between the small cylinder and large cylinder, and between the latter and the condenser, must all be shut off; and the communications must be opened between the two sides of the smaller piston and between the two sides of the larger piston: then, as the pistons ascend, the steam which was last admitted above the small piston will pass to its under side, while the steam which was last admitted to the top of the large from the lower part of the small cylinder will pass into the lower part of the large cylinder under its piston, and both pistons will at the same time ascend by the action of the counterweight, or the momentum of the fly, as the case may be. When both pistons have reached the top of their respective cylinders, the last-mentioned communications are shut, and the others are opened as before, and both will descend; the steam which was last admitted to the under side of the small piston passing into the upper end of the large cylinder, while the steam last introduced below the piston of the large cylinder goes off to the condenser. Or an arrangement similar to the following may be adopted: Let only the bottom part of the smaller cylinder have a communication with the boiler, and let there be also a communication between the bottom of the smaller and the top of the larger cylinder, and another communication between the top part of both cylinders, which last-mentioned communication always remains open; and let the top and bottom of the large cylinder also communicate by means of a side pipe; and let the bottom part of the large cylinder communicate with a condenser. Things being thus arranged, when steam is admitting into the lower part of the smaller cylinder, the piston in that cylinder will ascend; the communication between the top of the two cylinders being then open, to allow the steam above the smaller piston to pass into the top of the larger, where it is not to be detained, but, along with the steam above the piston in the large cylinder, to be allowed to pass under the last-mentioned piston (which is now ascending as well as the piston in the smaller cylinder), the communication between the two sides of the large piston being now open, and the communication with the condenser shut off. Both pistons having reached the top of their respective cylinders, the communications which before were open must now be shut (excepting the communication between the two cylinders at their upper part), and the communications between the bottom of the small and the top of the large, and between the bottom of the

large

large and the condenser, must be opened: the steam last admitted into the lower part of the smaller cylinder will now ascend into the top of the larger and expand itself, following the piston of the larger cylinder which is now descending, its other side being open to the condenser; and (as the tops of the two cylinders communicate) steam will also pass into the top of the smaller cylinder above its piston, so that both pistons descend together to the bottom of their respective cylinders; when a fresh charge of steam is again to be admitted for a fresh stroke of the engine.

4thly. If my improvements be applied to one of the engines known by the name of Watt's double engine, the working cylinder must have no direct communication with the boiler, but must be made to communicate at its top and bottom alternately with the lower and upper part of the smaller added cylinder, or simply, as the case may be, at its top and bottom, with the steam measure, which has a direct communication with the boiler; the effect of which must be sufficiently obvious from the details I have already given respecting the nature of my invention, and the way in which the same is to be carried into actual practice.

5thly. With regard to steam-engines, in which the separate steam measure may not be thought advisable, the same may be improved by the application of my aforesaid discovery, by making the boiler, and the steam case or cases in which the working cylinder or cylinders is or are inclosed, much stronger than usual; and by altering the structure and dimensions of the cocks, valves, slides, or other means of admitting steam from the boiler into the cylinder or cylinders, in such a manner as that the steam may be admitted very gradually by a progressive enlargement of the aperture, so as at first to wiredraw, and afterwards admit more freely the same. The reason for this precaution is this: Steam of such great elastic force as I employ, if admitted suddenly into the cylinder or cylinders, when more than one working cylinder is employed, would strike with a force that would endanger the safety and durability of the engine. The aperture allowed to the valve, cock, or other contrivance for admitting steam into the cylinder or cylinders, should be regulated by the following consideration:— If the intention is that the engine should work only, or almost wholly, by condensation, the steam in passing into the cylinder or cylinders should be forced to wiredraw itself, as I have already said, but so that the piston or pistons may perform the whole or a great part of the stroke by the time the intended quantity of steam has been admitted into

the cylinder or cylinders: for example, when steam of forty pounds the square inch is used, such a quantity of the same as shall be equal in volume to one-fortieth of the capacity of the cylinder or cylinders, and so in proportion when steam of any other force is employed; and when the requisite quantity has been admitted, the steam is to be shut off till the proper moment for admitting a fresh quantity. But if it is intended that advantage shall also be taken of the elastic force of the steam acting on one side of the piston or pistons, while condensation goes on on the other side, then the steam must be admitted more freely, but still with caution at the first, for the reason already mentioned. And in this, as well as in every other application of my said improvements, grounded on my said discovery of the law of expansibility of steam, due and effectual means must be used to keep up the requisite temperature of all the parts of the apparatus into which the steam is admitted, and in which it is not intended to be condensed. And here it may be proper to state, that, instead of the obvious and usual means for accomplishing this, namely, inclosing them in the boiler, or in a steam case or cases communicating with the boiler, the following method may sometimes be used with advantage, viz. a separate fire under the steam case or cases, which, in that event, will become a boiler or boilers, and must be furnished with a safety-valve or valves to regulate the temperature. By means of the last-mentioned arrangement, the steam from the smaller cylinder or steam measure (when either of these is employed) may be admitted into the larger cylinder or cylinders kept at a higher temperature than the smaller, by which its power to expand itself may be increased; and, on the contrary, by keeping the larger at a lower temperature than the smaller, its expansibility will be lessened; which, on particular occasions, and for particular purposes, may be desirable. In every case care must be taken that the boiler, the case or cases in which the cylinder or cylinders is or are inclosed, the steam pipes, and generally all the parts exposed to the action of the expansive force of the steam, be of a strength proportioned to the high pressure to which they are to be exposed. With regard to the due degrees of strength of the parts of all my said improvements, together with the nature of the materials, and the proportions of the same, except as herein described, I forbear any further description, on account of the numerous variations to which the same must be subjected; and because every engineer, of sufficient skill to be employed in works of this nature, will,

without difficulty, arrange and determine the same according to the nature of the case.

Lastly. I have to observe, that, to avoid unnecessary tautologies in speaking of the proportion that ought to subsist between the smaller cylinder or the steam measure and the larger cylinder, or the quantity of steam to be admitted to the working cylinder, where a separate smaller cylinder or steam measure is not adopted, though I have mentioned the regular proportions, as, for example, of a measure or quantity equal to one-fortieth of the working cylinder when steam of forty pounds the square inch is to be employed, or equal to one-thirtieth, or one-twentieth, when steam of thirty or twenty pounds the square inch is to be used; yet these are not the only proportions that may be used; for, though it may not be advisable that the proportion of the smaller cylinder or steam measure should in any case be made much smaller than I have stated, yet, in making it larger, considerable latitude may be allowed: for example, with steam of forty pounds the square inch, a smaller cylinder or measure of one-twentieth, or even larger, or of some intermediate proportion, may be employed instead of one of a fortieth of the capacity of the larger or working cylinder; and so with steam of any other given strength. And it may be advisable, that in a number of engines this should be the case, because of the difficulty of preventing some waste of steam or partial condensation which might lessen the rate of working, if not allowed for in the size of the smaller cylinder or steam measure; or in the quantity of steam admitted directly from the boiler into the working cylinder, where no smaller cylinder or steam measure is employed: and in every case the engine, when got ready for work, whatever may be the proportion that has been adopted as intended to be worked with, should have its power tried, by altering the load on the valve that ascertains the force of the steam, in order that the strength of steam best adapted for the engine may be ascertained; for it may turn out to be advantageous that the steam should be employed, in particular engines of an elastic force, somewhat over or under what was first intended.

In witness whereof, &c.

LXV. *On*

LXV. *On the Chemical Nature of Blighted Corn. Extracted from a Memoir read at the Institute 30th Vendemiaire, Year 12. By Messrs.* FOURCROY *and* VAUQUELIN*.

THE blight in corn has already engaged the attention of many chemists. Parmentier found in it a fœtid greasy substance mixed with charcoal; Cornette has discovered its oily nature; and M. Girod-Chautrans has announced, in the year 12, the existence of an acid both free and combined, which he conceived to be of a peculiar nature.

It is since this last discovery was announced to the Institute by its author, towards the end of the year 12, that M. Vauquelin and I applied ourselves to examine with accuracy this diseased vegetable matter.

The blight, it is known, is, in fact, a disease of the grain which presents under the husk of the seed, where the farinaceous substance ought to be found, a black powder, greasy and fœtid, distinguished by the dangerous quality of infecting other grain by contact, and communicating to it the property of growing up blighted. The most effectual means of destroying the contagion, and preventing the disease from re-appearing, is, to wash the grain with lime water, or an alkaline solution: without this precaution, which is now in general use among all well-informed husbandmen, the disease is uniformly reproduced.

The blighted grain on which our experiments were made was furnished by M. Girod-Chautrans, whose zeal for the advancement of science, and enlightened love of natural history, are well known.

When pounded in a mortar of flint, and separated from its husk, the blight communicated to hot alcohol a greenish yellow colour, but no acid property; and there was separated a dark green oily matter of the consistence of butter, and acrid like rancid fat. Ether, also, produced the separation of the same oil.

The substance, after being thus acted on by the alcohol, still preserved its greasy feel, and its peculiar odour of sea-ware. When washed with five times its weight of boiling water, the water assumed a reddish brown colour, a fœtid odour, somewhat of a soapy quality, and a decidedly acid character. When examined by different appropriate reagents, the acid was proved to be the phosphoric.

* From *Annales du Muséum d'Histoire Naturelle*, No. 35.

By

By washing the pure blight with boiling distilled water, without treating it with alcohol, the liquor became sensibly acid, and when saturated with potash yielded a precipitate of an animal matter mixed with phosphate of ammonia and magnesia, at the same time exhibiting every mark of the presence of an alkaline phosphate. Thus have these experiments confirmed the opinion that phosphoric acid exists uncombined in blight; for we have a sufficient proof of this in its fixity, its insolubility in alcohol, its solubility in water, and its precipitation by lime water, &c.

The water held in solution, after the acid had been precipitated by potash, a fœtid animal matter, altogether similar in its colour, odour, and in the phænomena of its precipitation by different re-agents, to that which is found in water where the gluten of flour has been left to putrefy.

After the successive action of the alcohol and water, the blight of wheat still retained its fœtid odour and greasy feel. When distilled with a naked fire, it yielded one-third of its weight of water containing acidulous acetate of ammonia; nearly one-third of a dark brown concrete oil, very similar to adipocère in its form, consistence, and easy fusibility; also 0·23 of a charcoal, which left, by incineration, a white cinder weighing one-hundredth part of the original quantity of blight, and consisting of three-fourths phosphate of magnesia and one-fourth phosphate of lime.

We examined blight without removing the husk, and found that this had no influence in varying the ultimate result of the analysis.

We would conclude, from the examination which we have just detailed, that the blight of wheat contains,

1. A green oil, having the consistence of butter, fœtid and acrid, soluble in heated alcohol and ether; forming nearly a third of the weight of the blight, and communicating to it its greasy feel.

2. A vegeto-animal matter soluble in water, insoluble in alcohol, and precipitating the greater number of the metallic salts as well as nut-galls. It forms rather less than a fourth part of the blight, and resembles completely the substance which is produced by the putrefaction of gluten.

3. A charcoal, amounting to a fifth part, which gives a black colour to the whole mass, and is at once the proof and the effect of a putrefactive decomposition; as it is also in garden mould, and indeed in the remains of all putrefied organic compounds.

4. Phosphoric acid in a free state, forming only 0·001 of the

the blight, but sufficing to give it the power of reddening vegetable blues.

5. Lastly, The phosphates of ammonia, of magnesia, and lime, in the small proportion of some thousand parts.

Thus the blight of wheat is only a residuum of putrefied farina, which, instead of the constituent elements of this last, viz. starch, gluten, saccharine matter, contains only a kind of charred oily substance very similar to that species of bitumen which derives its origin from animal or vegeto-animal bodies.

We would here call to mind, that properties very similar to those of the blight of wheat were presented to us during our examination of gluten decomposed by putrefaction, and that the products of the one approach so near to those of the other, as to render it difficult, in some cases, to avoid confounding them. Indeed, it requires considerable experience in chemical operations to discover the slight differences existing between these two putrefied matters; for they consist only in those slight shades which are caught with difficulty.

However interesting the results of this analysis may appear, we must admit that the knowledge which they afford regarding the nature of blight serves to throw but little light on its cause, or its contagious quality; which is now proved by so many experiments, that there can no longer remain the slightest doubt: we must also confess that these results, while they show blight to consist of the residuum of putrefied farina, do not completely accord with the ideas of those philosophical agriculturists who regard this disease as the necessary effect of contagion; for, agreeably to our analysis, it would seem equally reasonable to consider it as the effect of putrefactive decomposition, which may arise from many other circumstances besides the influence of infection *.

These results induce us also to think that the septic process, which necessarily precedes the formation of blight in every case, whether arising from contagions or occurring spontaneously, attacks in particular the gluten, and anticipates, nay, prevents the formation of starch; for we well know that this matter, of which no traces are discoverable in blight, suffers no alteration from the putrefaction which attacks so violently the substance of gluten.

* From sir Joseph Banks's paper which we have laid before our readers, we have a right to consider blight as a vegetable fungus. If this be admitted, it is easy to conceive that, the organization of the grain being destroyed by the shooting of the roots of the fungi, a slow decomposition of the seed must follow.—Edit.

LXVI. *Ex-*

LXVI. *Experimental Inquiry into the Proportion of the several Gases or Elastic Fluids constituting the Atmosphere.* By JOHN DALTON*.

IN a former paper which I submitted to this society, "on the constitution of mixed gases," I adopted such proportions of the simple elastic fluids to constitute the atmosphere as were then current, not intending to warrant the accuracy of them all, as stated in the said paper: my principal object in that essay was, to point out the *manner* in which mixed elastic fluids exist together, and to insist upon what I think a very important and fundamental position in the doctrine of such fluids;—namely, that the elastic or repulsive power of each particle is confined to those of its own kind; and consequently the force of such fluid, retained in a given vessel, or gravitating, is the same in a separate as in a mixed state, depending upon its proper density and temperature. This principle accords with all experience, and I have no doubt will soon be perceived and acknowledged by chemists and philosophers in general; and its application will elucidate a variety of facts which are otherwise involved in obscurity.

The objects of the present essay are,

1. To determine the weight of each simple atmosphere, abstractedly; or, in other words, what part of the weight of the whole compound atmosphere is due to azote, what to oxygen, &c. &c.

2. To determine the relative weights of the different gases in a given volume of atmospheric air, such as it is at the earth's surface.

3. To investigate the proportions of the gases to each other, such as they ought to be found at different elevations above the earth's surface.

To those who consider the atmosphere as a chemical compound, these *three* objects are but *one*; others, who adopt my hypothesis, will see they are essentially distinct. With respect to the first: it is obvious, that, on my hypothesis, the density and elastic force of each gas at the earth's surface are the effects of the weight of the atmosphere of that gas solely, the different atmospheres not gravitating one upon another; whence the first object will be obtained by ascertaining what share of elastic force is due to each gas in a given volume of the compound atmosphere; or, which

* From *Manchester Transactions*, second series, vol. i.

amounts

amounts to the same thing, by finding how much the given volume is diminished under a constant pressure, by the abstraction of each of its ingredients singly. Thus, if it should appear that, by extracting the oxygenous gas from any mass of atmospheric air, the whole was diminished 1-5th in bulk, still being subject to a pressure of 30 inches of mercury; then it ought to be inferred that the oxygenous atmosphere presses the earth with a force of 6 inches of mercury, &c.

In order to ascertain the second point, it will be further necessary to obtain the specific gravity of each gas; that is, the relative weights of a given volume of each in a pure state, subject to the same pressure and temperature. For, the weight of each gas in any given portion of atmospheric air must be in the compound ratio of its force and specific gravity.

With respect to the third object, it may be observed, that those gases which are specifically the heaviest must decrease in density the quickest in ascending. If the earth's atmosphere had been a homogeneous elastic fluid of the same weight it is, but ten times the specific gravity, it might easily be demonstrated that no sensible portion of it could have arisen to the summits of the highest mountains. On the other hand, an atmosphere of hydrogenous gas, of the same weight, would support a column of mercury nearly 29 inches on the summit of Mount Blanc.

The several gases constantly found in every portion of atmospheric air, and in such quantities as are capable of being appreciated, are azotic, oxygenous, aqueous vapour, and carbonic acid. It is probable that hydrogenous gas also is constantly present, but in so small proportion as not to be detected by any test we are acquainted with: it must therefore be confounded in the large mass of azotic gas.

I. *Of the Weight of the Oxygenous and Azotic Atmospheres.*

Various processes have been used to determine the quantity of oxygenous gas.

1. The mixture of nitrous gas and air over water.
2. Exposing the air to liquid sulphuret of potash or lime, with or without agitation.
3. Exploding hydrogen gas and air by electricity.
4. Exposing the air to a solution of green sulphat or muriat of iron in water strongly impregnated with nitrous gas.
5. Burning phosphorus in the air.

In

In all these cases the oxygen enters into combination and loses its elasticity; and if the several processes be conducted skilfully, the results are precisely the same from all. In all parts of the earth, and at every season of the year, the bulk of any given quantity of atmospheric air appears to be reduced nearly 21 per cent. by abstracting its oxygen. This fact, indeed, has not been generally admitted till lately; some chemists having found, as they apprehended, a great difference in the quantity of oxygen in the air at different times and places; on some occasions 20 per cent. and on others 30 and more, of oxygen are said to have been found. This I have no doubt was owing to their not understanding the nature of the operation, and of the circumstances influencing it. Indeed, it is difficult to see, on any hypothesis, how a disproportion of these two elements should ever subsist in the atmosphere.

As the first of the processes above mentioned has been much discredited by late authors, and as it appears from my experience to be not only the most elegant and expeditious of all the methods hitherto used, but also as correct as any of them when properly conducted, I shall, on this occasion, animadvert upon it.

1. Nitrous gas may be obtained pure by nitric acid diluted with an equal bulk of water poured upon copper or mercury; little or no artificial heat should be applied.—The last product of gas this way obtained does not contain any sensible portion of azotic gas; at least, it may easily be got with less than 2 or 3 per cent. of that gas. It is probably nearly free from nitrous oxide also, when thus obtained.

2. If 100 measures of common air be put to 36 of pure nitrous gas in a tube 3-10ths of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 measures, and exhibit no signs of either oxygenous or nitrous gas.

3. If 100 measures of common air be admitted to 72 of nitrous gas in a wide vessel over water, such as to form a thin stratum of air, and an immediate momentary agitation be used, there will, as before, be found 79 or 80 measures of pure azotic gas for a residuum.

4. If, in the last experiment, *less* than 72 measures of nitrous gas be used, there will be a residuum containing oxygenous gas; if *more*, then some residuary nitrous gas will be found.

These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of

of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former case nitric acid is the result; in the latter nitrous acid: but as both these may be formed at the same time, one part of the oxygen going to one of nitrous gas, and another to two, the quantity of nitrous gas absorbed should be variable; from 36 to 72 per cent. for common air. This is the principal cause of that diversity which has so much appeared in the results of chemists on this subject. In fact, all the gradation in quantity of nitrous gas from 36 to 72 may actually be observed with atmospheric air of the same purity: the wider the tube or vessel the mixture is made in, the quicker the combination is effected; and the more exposed to water, the greater is the quantity of nitrous acid, and the less of nitric, that is formed.

To use nitrous gas for the purpose of eudiometry, therefore, we must attempt to form nitric acid or nitrous wholly, and without a mixture of the other. Of these the former appears from my experiments to be most easily and most accurately effected. In order to this, a narrow tube is necessary; one that is just wide enough to let air pass water without requiring the tube to be agitated, is best. Let little more nitrous gas than is sufficient to form nitric acid be admitted to the oxygenous gas; let no agitation be used; and as soon as the diminution appears to be over for a moment, let the residuary gas be transferred to another tube, and it will remain without any further diminution of consequence. Then $\frac{7}{19}$ths of the loss will be due to oxygen.— The transferring is necessary to prevent the nitric acid formed and combined with the water, from absorbing the remainder of the nitrous gas to form nitrous acid.

Sulphuret of lime is a good test of the proportion of oxygen in a given mixture, provided the liquid be not more than 20 or 30 per cent. for the gas (atmospheric air): if the liquid exceed this, there is a portion of azotic gas imbibed somewhat uncertain in quantity.

Volta's eudiometer is very accurate as well as elegant and expeditious: according to Monge, 100 oxygen require 196 measures of hydrogen; according to Davy, 192; but from the most attentive observations of my own, 185 are sufficient. In atmospheric air I always find 60 per cent. diminution when fired with an excess of hydrogen; that is, 100 common air with 60 hydrogen become 100 after the explosion, and no oxygen is found in the residuum: here 21 oxygen take 39 hydrogen.

2. Of

2. *Of the Weight of the Aqueous Vapour Atmosphere.*

I have, in a former essay, (Manchester Mem. vol. v. p. 2, page 559,) given a table of the force of vapour in vacuo for every degree of temperature, determined by experiment, and in the sequel of the essay have shown that the force of vapour in the atmosphere is the very same as in vacuo, when they are both at their utmost for any given temperature. To find the force of aqueous vapour in the atmosphere, therefore, we have nothing more to do than to find that degree of cold at which it begins to be condensed, and opposite to it in the table above mentioned will be found the force of vapour. From the various facts mentioned in the essay, it is obvious, that vapour contracts no chemical union with any of the gases in the atmosphere; this fact has since been enforced in the Annales de Chimie, vol. xlii. by Clement and Desorme.

M. De Saussure found by an excellent experiment, that dry air of 64° will admit so much vapour as to increase its elasticity $\frac{1}{54}$th.—This I have repeated nearly in his manner, and found a similar result. But the table he has given us of aqueous vapour at other temperatures is very far wrong, especially at temperatures distant from 64°.—The numbers were not the result of direct experiment, like the one above.—If we could obtain the temperatures of all parts of the earth's surface, for any given time, a mean of them would probably be 57° or 58°. Now, if we may suppose the force of vapour equivalent to that of 55°, at a medium, it will, from the table, be = to ·443 of mercury, or nearly $\frac{1}{70}$th of the whole atmosphere. This, it will be perceived, is calculated to be the weight of vapour in the whole atmosphere of the earth. If that incumbent over any place at any time be required, it may be found as directed above.

3. *Of the Weight of the Carbonic Acid Atmosphere.*

From some observations of Humboldt, I was led to expect about $\frac{1}{100}$dth part of the weight of the atmosphere to be carbonic acid gas: but I soon found that the proportion was immensely overrated. From repeated experiments, all nearly agreeing in their results, and made at different seasons of the year, I have found, that if a glass vessel filled with 102,400 grains of rain water be emptied in the open air, and 125 grains of strong lime water be poured in, and the mouth then closed; by sufficient time and agitation, the whole of the lime water is just saturated by the acid gas

gas it finds in that volume of air. But 125 grains of the lime water used require 70 grain measures of carbonic acid gas to saturate it; therefore, the 102,400 grain measures of common air contain 70 of carbonic acid, or $\frac{1}{1460}$th of the whole. The weight of the carbonic acid atmosphere then is to that of the whole compound as 1 : 1460; but the weight of carbonic acid gas in a given portion of air at the earth's surface, is nearly $\frac{1}{1000}$dth of the whole; because the specific gravity of the gas is $1\frac{1}{2}$ that of common air. I have since found that the air in an assembly, in which two hundred people had breathed for two hours, with the windows and doors shut, contained little more than 1 per cent. of carbonic acid gas.

Having now determined the force with which each atmosphere presses on the earth's surface, or in other words its weight, it remains next to inquire into their specific gravities.

These may be seen in the following table.

Atmospheric air - -	1·000
Azotic gas - - -	·966
Oxygenous gas - -	1·127
Carbonic acid gas -	1·500
Aqueous vapour -	·700
Hydrogenous gas - -	·077*

Kirwan and Lavoisier are my authorities for these numbers, except oxygenous gas and aqueous vapour. For the former I am indebted to Mr. Davy's Chemical Researches; his number is something greater than theirs: I prefer it, because, being determined with at least equal attention to accuracy with the others, it has this further claim for credit, that 21 parts of gas of this specific gravity, mixed with 79 parts of azotic gas, make a compound of exactly the same specific gravity as the atmosphere, as they evidently ought to do, setting aside the unfounded notion of their forming a chemical compound. The specific gravity of aqueous vapour I have determined myself, both by analytic and synthetic methods, after the manner of De Saussure; that is, by abstracting aqueous vapour of a known force from a given quantity of air, and weighing the water obtained—and admitting a given weight of water to dry air, and comparing the loss with the increased elasticity. De Saussure makes the specific gravity to be ,71 or ,75; but

* The specific gravity of hydrogen must be rated too low: if 100 oxygen require 185 hydrogen by measure, according to this 89 oxygen would require only 11 hydrogen to form water; whereas 85 require 15. Hydrogen ought to be found about 1-20th part of the weight of common air.

be

he used caustic alkali as the absorbent, which would extract the carbonic acid as well as the aqueous vapour from the air. From the experiments of Pictet and Watt, I deduce the specific gravity of aqueous vapour to be ,61 and ,67 respectively. Upon the whole, therefore, it is probable that ,7 is very nearly accurate.

We have now sufficient data to form tables answering to the two first objects of our inquiry.

I. *Table of the Weights of the different Gases constituting the Atmosphere.*

	Inch. of mercury.
Azotic gas - - -	23·36
Oxygenous gas - - -	6·18
Aqueous vapour - -	·44
Carbonic acid gas - -	·02
	30·00

II. *Table of the proportional Weights of the different Gases in a given Volume of Atmospheric Air, taken at the Surface of the Earth.*

	per cent.
Azotic gas - - -	75·55
Oxygenous gas - -	23·32
Aqueous vapour - -	1·03*
Carbonic acid gas -	—·10
	100·00

III. *On the Proportion of Gases at different Elevations.*

M. Berthollet seems to think that the lower strata of the atmosphere ought to contain more oxygen than the upper, because of the greater specific gravity of oxygenous gas, and the slight affinity of the two gases for each other. (See Annal. de Chimie, tom. xxxiv. p. 85.) As I am unable to conceive even the possibility of two gases being held together by affinity, unless their particles unite so as to form one centre of repulsion out of two or more (in which case they become one gas), I cannot see why rarefaction should either decrease or increase this supposed affinity. I have little doubt, however, as to the fact of oxygenous gas observing a diminishing ratio in ascending; for, the atmospheres being independent on each other, their densities at different heights must be regulated by their specific gravi-

* The proportion of aqueous vapour must be understood to be variable for any one place: the others are permanent, or nearly so.

ties. Hence, if we take the azotic atmosphere as a standard, the oxygenous and the carbonic acid will observe a decreasing ratio to it in ascending, and the aqueous vapour an increasing one. The specific gravity of oxygenous and azotic gases being as 7 to 6 nearly, their diminution in density will be the same at heights reciprocally as their specific gravities. Hence it would be found, that at the height of Mount Blanc (nearly three English miles), the ratio of oxygenous gas to azotic, in a given volume of air, would be nearly as 20 to 80:—consequently it follows that at any ordinary heights the difference in the proportions will be scarcely if at all perceptible*.

LXVII. *Experiments on the Torpedo, by Messrs.* HUMBOLDT *and* GAY-LUSSAC. *Extracted from a Letter from M.* HUMBOLDT *to M.* BERTHOLLET, *dated Rome,* 15 *Fructid. Year* 13 †.

THE phænomena of electric fish ought to be the subject of our renewed researches, with a view to the opinion of many philosophers, who conceive that they are capable of being explained upon the principles of that beautiful theory with which Volta has enriched science. You well know, my respected friend, what must have been our anxiety to procure the torpedo; and you will perhaps be astonished that I should be so long in writing to you respecting it. At Genoa we found some of them, but we were then without instruments. At Civita Vecchia we searched for them in vain. At last, during our stay at Naples, we procured them very frequently, of great size, and very vigorous. I shall relate to you, in this letter, the series of experiments which M. Gay-Lussac and I have instituted upon the action of the torpedo (Raja Torpedo of Linnæus). M. de Buch, a German mineralogist, was present at our experiments. I present to you the results which they afforded, and relate the facts without introducing any theoretical notions.

Our experiments were principally intended to discover those conditions in which the torpedo is unable to communicate

* Air brought from the summit of Helvelyn, in Cumberland (1100 yards above the sea—barometer being 26,60), in July 1804, gave no perceptible difference from the air taken in Manchester. M Gay-Lussac determines the constitution of air brought from an elevation of four miles to be the same as that at the earth's surface.

† From *Annales de Chimie*, no. 166.

nicate those shocks termed electric, although the feeling is very different from that which is occasioned by the discharge of a Leyden phial. Having at hand no other work besides that of Aldini, in which he has combined into one view the beautiful researches of Geoffroy with those of Spallanzani and Galvani, we shall not have it in our power to compare our own labours with those of preceding philosophers.

1. Although the power of the torpedo cannot be compared to that of the gymnotus, it is not less capable of occasioning disagreeable sensations. A person much in the custom of receiving electric shocks, supports with some difficulty the shock of a torpedo 14 inches long, and in a state of perfect vigour. The gymnotus communicates its influence under water, unless when much weakened.

M. Gay-Lussac has observed, that the action of the torpedo in this condition is not perceptible until it is raised above the surface of the water. It is with this fish, as with frogs on which Galvanic experiments are made: the circumstances under which the contraction takes place, vary according to the degree of excitability in the organs.

2. I have remarked, while in South America, that the gymnotus gives the most frightful shocks without making any external motion of the eyes, head, or fins: it moves no more than one person communicating an idea or sensation to another. But the torpedo, on the contrary, moves its pectoral fins in a convulsive manner before each shock; and the violence of the stroke is always proportioned to the extent of the surface of contact.

3. The organs of the torpedo, or gymnotus, cannot be discharged by us at will, like a Leyden phial or a Galvanic pile; nor does the electric fish uniformly communicate a shock when touched. It must be irritated that it may give its stroke; for this action depends upon the will of the animal, which in all probability does not always keep its electric organs charged: it charges them, however, with astonishing celerity, and is thus able to give a long series of shocks.

4. The shock is felt, provided the animal is disposed to give it, when a single finger is applied to a single surface of the electric organs; or when the two hands are placed one on the upper and the other on its under surface at the same time. And in either of these cases the shock is equally communicated, whether the person be insulated or not.

5. If a person while insulated touches the torpedo with his finger, it is indispensably necessary that the finger be in

immediate contact; for no shock is communicated when a conducting body, such as a piece of metal, is interposed between the finger and the organs of the fish. Thus we may touch the animal with a key, or other metallic instrument, and experience no shock in consequence.

6. M. Gay-Lussac having observed this important fact, we placed the torpedo on a plate of metal, so that the inferior surface of its electric organs was in contact with the metal. The hand which supported the plate felt no shock, although another person in a state of insulation irritated the animal, and when the convulsive motions of its pectoral fins plainly indicated very powerful discharges of its electric fluid.

7. If on the contrary a person support the torpedo placed on a metallic plate, with his left hand as in the preceding experiment, and with his right touches the upper surface of the electric organ, then a violent shock is felt in both arms at the same moment.

8. The same feeling is experienced when the fish is placed between two plates of metal, the edges of which do not touch, and the person applies a hand to each plate at the same instant.

9. But if in the preceding experiment there exists an immediate communication between the edges of the two plates, no shock is felt in the arms; for in this case the chain of connection between the two surfaces of the organ is formed by the plates, and the new communication established by applying the two hands to the plates becomes altogether inefficient.

10. The most delicate electrometer does not indicate the state of electricity of the organs of the torpedo: it is no way affected by any method which we can have recourse to, either by bringing it near to the organs, or by insulating the fish, covering it with a plate of metal, and then forming a communication by means of a wire between the plate and the condenser of Volta. Nothing shows here, as in the gymnotus, that the animal can modify the state of electricity of surrounding bodies.

11. As electric fish act while in a state of health with the same power under water as in the air, we examined the conducting properties of this fluid. A number of persons having formed a circle of connection between the upper and under surfaces of the organs of the torpedo, no shock was experienced till they had moistened their hands with water. The shock is equally felt when two persons who have their right hands applied to the torpedo, instead of taking hold of

each other's left hands, plunge a pointed piece of metal into a drop of water placed upon an insulating body.

12. By substituting flame instead of a drop of water, the communication is interrupted, and no sensation is experienced until the two pieces of metal touch each other within the flame.

13. We must also observe that no shock will take place either in air or under water, unless we immediately touch the body of the electric fish. They are unable to give their stroke through a layer of water, however thin; a fact which is the more remarkable, as we know that in Galvanic experiments, where the frog is placed under water, it is sufficient to bring the silver forceps near to the muscles, and that the contraction takes place when the layer of water interposed is one or two millimetres in thickness.

Such, my respected friend, are the principal observations which we have made upon the torpedo. Experiments 4th and 10th prove that the electric organs of these animals manifest no tension or excess of charge. We should rather be inclined to compare their action to a chain of small Leyden phials than to the pile of Volta. As some communication is always necessary for the occurrence of a shock, and having received strokes from the gymnotus through very dry cords, I conclude that in the case where this powerful animal appeared to give these violent shocks without the existence of any communication, it must have arisen from my imperfect insulation. If the torpedo acts by poles, by an electrical equilibrium which tends to re-establish itself, experiments 5th and 6th seem to prove that these poles exist near one another upon the same surface of the organ: for we feel the shock by touching a single surface with the finger. A plate interposed between the hand and the organ (6.) re-establishes of itself the equilibrium, and the hand which supports this plate feels nothing, because it is out of the current of the electric influence. But if we suppose a number of poles of different descriptions upon each surface of the organ, why is it that, by covering these with two metallic plates the edges of which do not touch, and placing the hands upon these plates, the equilibrium is re-established through the medium of the arms? Wherefore, it may be asked, does not the positive electricity of the inferior surface seek, at the moment of explosion, the negative electricity of the neighbouring pole? and wherefore does it find it only in the superior surface of the electric organ? These difficulties are perhaps not in-

 surmountable,

sumountable, but the theory of these *vital actions* requires still further research. Geoffroy* has proved that rays, which do not exhibit any marks of electricity, possess organs very analogous to those of the torpedo. The least injury of the brain of this animal prevents its electrical action. The nerves, therefore, without doubt, act the chief part in the production of these phænomena; and the physiologist, who takes a general and enlarged view of the vital actions, would with reason oppose the ideas of the philosopher, who conceives he can explain the whole by the contact of the albumino-gelatinous pulp with the tendinous septa which nature has combined in the formation of the organs of the torpedo.

LXVIII. *Notices respecting New Books.*

The Philosophical Transactions of the Royal Society of London for 1805, Part II., contain the following papers: 1. Abstract of Observations on a Diurnal Variation of the Barometer between the Tropics. By J. Horsburgh, Esq.—2. Concerning the Difference in the Magnetic Needle, on board the Investigator, arising from an Alteration in the Direction of the Ship's Head. By Matthew Flinders, Esq. Commander of His Majesty's Ship Investigator.—3. The Physiology of the Stapes, one of the Bones of the Organ of Hearing; deduced from a comparative View of its Structure and Uses in different Animals. By Anthony Carlisle, Esq. F. R. S.—4. On an Artificial Substance which possesses the principal characteristic Properties of Tannin. By Charles Hatchett, Esq. F.R.S.—5. The Case of a full grown Woman in whom the Ovaria were deficient. By Mr. Charles Pears, F.L.S.—6. A Description of Malformation in the Heart of an Infant. By Mr. Hugh Chudleigh Standart.—7. On a Method of analysing Stones containing fixed Alkali, by means of the Boracic Acid. By Humphrey Davy, Esq. F.R.S.—8. On the Direction and Velocity of the Motion of the Sun and Solar System. By William Herschel, LL.D. F.R.S.—9. On the Reproduction of Buds. By Thomas Andrew Knight, Esq. F.R.S.—10. Some Account of Two Mummies of the Egyptian Ibis, one of which was in a remarkably perfect

* For M. Geoffroy's paper, see Phil. Mag. vol. xv. p. 126.

State.

State. By John Pearson, Esq. F.R.S.—11. Observations on the singular Figure of the Planet Saturn. By William Herschel, LL.D. F.R.S.—12. On the Magnetic Attraction of Oxides of Iron. By Timothy Lane, Esq. F.R.S.—13. Additional Experiments and Remarks on an Artificial Substance, which possesses the principal characteristic Properties of Tannin. By Charles Hatchett, Esq. F.R.S.—14. On the Discovery of Palladium, with Observations on other Substances found with Platina. By William Hyde Wollaston, M.D. F.R.S.—15. Experiments on a Mineral Substance, formerly supposed to be Zeolite, with some Remarks on Two Species of Uran-glimmer. By the Rev. William Gregor.

Several of the above papers have been laid before our readers in our last and in the present number.

The Royal Society of Edinburgh has published the conclusion of its 5th volume. It contains the following papers: 1. On the Origin and radical Sense of the Greek Prepositions. By Mr. James Bonner.—2. Experiments on the Contraction of Water by Heat, at low Temperatures. By Dr. Thomas Charles Hope (published in our present volume, p. 153).—3. The History of the Society, consisting of the following Articles:—Diurnal Variations of the Barometer. By Mr. Playfair.—Aurora Borealis observed in Day-Light. By the Rev. Dr. Patrick Graham.—Phænomenon of Two Rainbows intersecting one another. By Mr. Playfair.—On the Combustion of the Diamond. By Sir George Mackenzie, Bart.—Remarks on the Basalts of the Coast of Antrim. By the Rev. Dr. Richardson.—Rule for reducing a Square Root by a continued Fraction. By James Ivory, Esq.—Singular Variety of Hernia. By Mr. Russel.—Concerning the Chartreuse of Perth. By the Abbé Mann.—Explanation of the Old Word Skull or Skoll. By the Rev. Dr. Jamieson.—Biographical Account of the late Dr. James Hutton. By Mr. Playfair.—Minutes of the Life and Character of Dr. Joseph Black. By Dr. Ferguson.—Appendix.—List of Members elected since the Publication of the last Volume.—List of Donations.

The Literary and Philosophical Society of Manchester, to which the philosophical world was before indebted for a number of valuable papers published at different periods, in five volumes, has just published another volume, being the first of a new series. Its contents are:—1. An Essay Physiological and Experimental on the Effects of Opium on the Living

Living System. By William Alexander, M.D.—2. On the Machinery of the Ancient Epic Poem. By the Rev. G. Walker, F.R.S.—3. Observations on the Effect of Madder Root on the Bones of Animals. By Mr. B. Gibson.—4. On the Use and Abuse of Popular Sports and Exercises, resembling those of the Greeks and Romans, as a national Object. By S. A. Bardsley, M.D.—5. Reverie; considered as connected with Literature; an Essay. By the Rev. Johnson Grant, A.B.—6. Experimental Inquiry into the Proportion of the several Gases or Elastic Fluids constituting the Atmosphere. By John Dalton.—7. On the Tendency of Elastic Fluids to Diffusion through each other. By John Dalton.—8. On the Absorption of Gases by Water and other Liquids. By John Dalton.—9. A Description of a Property of Caout-chouc or Indian Rubber; with some Reflections on the Cause of the Elasticity of this Substance. In a Letter to Dr. Holme. By Mr. John Gough.—10. An Essay on the Theory of mixed Gases, and on the State of Water in the Atmosphere. By Mr. J. Gough.—11. On the Use of the Sutures in the Skulls of Animals. By Mr. B. Gibson.—12. On the Moral Influence of History. By the Rev. G. Walker, F.R.S.—13. Reflections on History and on Historians Antient and Modern. By John Holland.—14. On Natural and Moral Philosophy; and the proper Manner of philosophizing in both. By the Rev. G. Walker, F.R.S.—15. A Reply to Mr. Dalton's Objections to a late Theory of mixed Gases. In a Letter from the Author to Dr. Holme. By Mr. John Gough.—16. Remarks on Mr. Gough's two Essays on the Doctrine of mixed Gases; and on Professor Schmidt's Experiments on the Expansion of Dry and Moist Air by Heat. By John Dalton.

M. Hermbstaedt, of Berlin, has published the prospectus of a work which he intends shortly to lay before the public. As this work will probably prove highly useful to calico printers, and dyers, we subjoin a translation of the

PROSPECTUS.

Having, for a period of six years, made the art of dyeing and printing the constant object of my labours, I have succeeded in discovering a considerable number of new mordants for producing the most beautiful and lasting colours for all sorts of cotton and linen goods; likewise in producing numerous colours for copper-plate printing, and a great variety, of all tints and shades, for pencilling, which, in

in regard to beauty and quality, will leave but little room for further exertions.

The time bestowed on these objects, and the expenses required for numberless trials, impose upon me the duty of endeavouring to obtain a moderate restitution for both. I have therefore resolved to publish my discoveries under the title of "Discoveries and Inventions for preparing new Mordants for Cotton and Linen, in order to produce beautiful and lasting Colours; with Instruction for producing genuine Colours both for Block and Copper-plate Printing, as also for Pencilling in all Shades."

The whole will be divided under the following heads:

1. Instructions for determining and preparing such substances as are required for the production of new mordants, and colours for printing and pencilling.

2. Instructions for compounding new mordants.

3. Instructions for compounding genuine and beautiful colours for printing in all tints and shades.

4. Instructions for compounding genuine and beautiful colours of all tints for pencilling.

To those who confide in my abilities and character, I offer this work, by way of subscription, at the price of three guineas; and at the expiration of March 1806 the subscription will be closed, when the printing of it will take place, and the price to non-subscribers will then be five guineas.

Berlin, Dec. 13, 1805. S. F. HERMBSTAEDT.

Subscribers in London, or other parts of Britain, are requested to apply, post paid, to Mr. J. Hunneman, No. 6, Frith-street, Soho, where subscriptions will be received, and proper receipts given. Such as are not masters of the German language, are informed that a translation of the above work will be undertaken by

J. HUNNEMAN.

LXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

On the evening of Thursday, the 9th instant, this society assembled after the Christmas holidays, the right honourable Sir Joseph Banks, bart. president, in the chair. A summary of Mr. Brandé's experiments on guaiacum, as mentioned in our last month's report, was read. In 100 grains of

of that substance, according to the analysis of this ingenious chemist, there were (as near as we can recollect) 22½ grains of water and oil; of empyreumatic oil about 30; carbon 30; lime 9; carbonated hydrogen gas about 8. In a postscript the author noticed a solution of guaiacum that had assumed the appearance of caout-chouc, but when reduced to the solid form it became brittle. Of the chemical accuracy of Mr. Brandé's experiments there cannot be the least doubt; but he has for the present rested content with giving only a general analysis, without entering into a detail of all the constituent principles of this vegetable product, to which he has very properly given the generic denomination of extracto-resin. From these experiments, and the more profound researches of the very accurate and indefatigable Mr. Hatchett, we may shortly expect to possess ample knowledge of the nature and properties of gums, resins, and balsams.

On the same evening commenced the reading of a letter from F. A. Knight, esq. to the right honourable Sir Joseph Banks, on the descent of the roots, and the elongation of the germs, of plants. To the right honourable President who directs, and the philosopher who executes these vegetable physiological researches, the public is deeply indebted; and Mr. Knight, perhaps with the exception of Hedwig, is almost the only botanist in Europe who seems to be convinced that botany is not a science but as it is connected with the physiology of vegetation, and the anatomy of vegetables. While other botanists are occupied with fruitless and disgusting logomachies on terminology, this naturalist modestly develops the progress of vegetation, the true philosophy of botany, with an ardour, originality, and accuracy of observation, that must eventually produce the most beneficial consequences to society. We would not deprive Mr. Knight of the honour of presenting the public with the result of his researches in his own terms, by anticipating the consequent advantages to agriculture in developing the best means of administering the most proper quantity and quality of food necessary to facilitate the growth and augment the size of those plants which are indispensable to animal existence.

January 16, the right honourable the President in the chair. Continuation of Mr. Knight's excellent paper on vegetable radiation and germination. To determine whether the descent of the roots of plants was the effect of an inherent principle, or the consequence of mechanical gravitation, the author placed different seeds in boxes attached

to

to two wheels, one of which was horizontal, and the other vertical, turned by a rivulet that runs through his garden. One of these wheels performed 150 and the other 250 rotations in a minute; a velocity which rendered the power of gravitation equal on both sides of the seed, and produced, as the author had expected, the protrusion of roots and elongation of germs in all directions. From several very decisive experiments to elucidate this fact, he concludes, contrary to Du Hamel, that gravitation is the sole cause of the descent of roots and ascent of germs, and that both diverge in all directions when under the influence of equal pressure. His observations also refute the too common notion that the roots of plants have an affinity for water, or rather that water attracts them from their accustomed direction by the nutriment which it affords. Mr. K. confirms, however, the generally received opinion of the affinity of plants for light. On the reputed tap-root of oaks he observes, that, after examining above 2000 trees, not one of which had such a root, he thinks he shall be pardoned for concluding that it has no existence. Of these interesting experiments we shall only observe, that, although the naturalist has carefully noted the time taken by the different seeds to germinate in the horizontal and vertical wheels, he has not mentioned what time would have been necessary for the same purpose in similar earth subject to the usual effect of gravitation. Such a comparison made with seeds of all the different qualities capable of vegetating, might develop some facts important to our present system of husbandry. On the same evening also commenced the reading of a "Third Communication on Artificial Tannin, by Mr. Hatchett."

January 23, the right honourable the President in the chair. Continuation of the above communication. Mr. Hatchett found that all gums, resins, and balsams, yield this artificial tanning substance on being treated with nitric acid, as observed in his preceding papers. It appears that all these substances yield at one operation a certain quantity of this matter; but if the process be continued too long the product is destroyed, the same as the natural tannin is by nitric acid. Mr. Hatchett was not able to ascertain either the specific quantity produceable at once, nor the precise time requisite to disengage the greatest possible quantity without injuring the entire product. The author, animated with this complete success on resinous substances, has pursued his researches on a vast variety of vegetable productions; whence it appears, that almost all vegetables, after

after being slightly carbonized and treated with nitric acid, yield this artificial tanning substance; and that oak-bark, deprived of its natural tannin in the tanners' pits, by being repeatedly charred or roasted, and as often subjected to nitric acid, is almost totally convertible into artificial tannin. Horse-chesnuts are also transmutable in a similar degree by the same process.

The reading of the conclusion of this paper was postponed till the 6th of February.

SOCIETY OF ANTIQUARIES.

The meetings and adjournments of this society being the same generally as those of the Royal, it of course assembled as usual on

January 9th, the vice-president, sir H. C. Englefield, bart. M. P. in the chair. A drawing of a spear-head found at Gringley Carr's Common, Nottingham, in 1803, was exhibited. This instrument is evidently of Roman origin; and from the circumstance of its having been so long in the earth without being discoloured or rusted, the learned antiquary supposed it to have been made of Corinthian brass. A similar implement is described in the ninth volume of the Archæologia, and considered as Roman manufacture. With regard to the peculiar qualities and composition of these instruments, a very learned and truly philosophical paper by Dr. G. Pearson, published in the Transactions of the Royal Society for 1796, will afford the most satisfactory information. From the very accurate chemical analysis of this philosopher, it appears that these Roman instruments, spear-heads and swords, found in various parts of England, are not brass as commonly alleged, but an alloy of copper and tin, in the proportion of about 10 parts of the former to one of the latter, the specific gravity of which was about 8·500. Geoffroy, indeed, after a number of experiments that might be any thing but chemical, flattered count Caylus by alleging that they were an alloy of copper and iron! Such an opinion might have been philosophical in the age of alchemy. From the researches of Dr. Pearson on this subject, a report was made by some French chemists, in 1801, a translation of which was published in the 10th volume, p. 340, of the Philosophical Magazine. There are many circumstances that render an intimate knowledge of this peculiar alloy so much in use with the Romans, highly interesting to the English antiquary. Doubtless the Romans, when they overran this country, taught the antient Britons how to manufacture this

this most useful metal. In that case, the antient importance and use of English tin must have been much greater than hitherto supposed. But there is a more modern use of this alloy, which antiquaries have almost wholly overlooked or very imperfectly understood, and to which we should beg leave to call their attention: we mean those engraved plates on tomb-stones, and sarcophagi, vulgarly called brass, and many of them actually are of that metal; but there are more, especially of those made in the 12th, 13th, and 14th centuries, of this gun-metal, or an alloy of copper and tin. The churches at St. Albans *, Watford, &c. and almost all parts of England, contain tombs decorated with these plates, supposed, perhaps erroneously, to be of Flemish manufacture. It is not improbable, indeed, that those which are made of brass, and which are almost entirely consumed with rust, may be of foreign origin, while those made of copper and tin have withstood the action of the humid atmosphere of vaults upwards of four centuries, without any indication of oxidation or consumption by rust.

On the same evening the indefatigable Mr. Lysons furnished some curious items respecting the origin and history of sugar. It appears that the antients had nothing but honey; and that, till about the year 620 of the present æra, sugar-cane was wholly unknown. The discovery is ascribed to a Venetian, who called it cane-honey. It is not above 200 years since refined sugar was first introduced into Europe from China by the Portuguese and English.

January 16, the vice-president, sir H. C. Englefield, bart. in the chair. Several curious extracts from his majesty's records in the Tower, by Mr. Lysons, were read, on the prices of drugs and medicines during the reigns of the first Edwards.

January 23, the right honourable the earl of Leicester, president, in the chair. A letter from Mary de Medicis, queen of France, to her daughter, Henrietta Maria, queen of Charles the First, was exhibited by Dr. Garthshore. It contained nothing particular, but merely a recommendation of the bearer to her daughter's attention. According to the usage of that period (1636) it was sealed with two seals united by a narrow tape or ribbon. Several other miscellaneous letters were read, but which were of a nature and importance that do not call for particular notice.

* See Brayley's Beauties of England and Wales.

PHILO-

PHILOSOPHICAL SOCIETY, BOROUGH OF SOUTHWARK.

It gives us pleasure at all times to be able to record the formation of societies for the diffusion of knowledge; and it is with much satisfaction we announce that one has been formed in the Borough which promises to prove useful. The society meets at the house of Mr. Snart, optician, 215 Tooley-street, a zealous promoter of, and diligent inquirer into, every thing connected with natural philosophy.

MUSEUM ALEXANDRINUM.

By letters from Petersburgh of the 22d December last, we are informed that a number of persons have united to establish a society under the above name, for the purpose of making a collection of works of art of *all kinds*, and to combine with it a collection of the best scientific works and journals. This society means to publish its transactions.

BATAVIAN SOCIETY OF SCIENCES, HAARLEM.

The following particulars are extracted from the program of the society for the year 1805:

1. The society have received, upon the question which requires "A memoir containing the principal facts that have been hitherto brought to light by the electric pile of Volta, and by the numerous experiments made to ascertain its effects," an answer in Dutch, having for motto *Men sticht gren eerezuil*, &c. They have adjudged that this memoir indicates strongly the talents of its author, but that it cannot receive the prize. They have therefore determined to repeat this question; the answer to be given in before the 1st of November 1806.

2. They have received upon the question—"What are the principles in the natural history of fire, respecting the production, communication, and confinement of heat, requisite to determine the most œconomical mode of employing fuel for the different purposes of heating; and how may we improve, according to these principles, chimneys for warming apartments, and stoves in kitchens, with a view to save, as much as possible, the fuel which is in common use with us?" a memoir in German, having for motto, *Inest in explicatione naturæ insatiabilis quædam e cognoscendis rebus voluptas*, &c. Cicero. This memoir appeared to correspond so exactly to the question proposed, that the prize was unanimously adjudged to it. On opening the letter which accompanied it, the author was found to be John Joseph Prechtl, of Brunn, in Moravia.

3. They have received upon the question,—"What is at present known of the causes which occasion the corruption of stagnant water? and are we able to discover, from the knowledge we possess upon this subject, or from any thing that can be proved by decisive experiments, the most effectual, and at the same time innocent, means of preventing the corruption of stagnant water?" two memoirs; the one in Dutch, having for motto, *Neque vero negligentiam*, &c. Hipp.; the other in French, having as motto, *Magnum artis partem*, &c. Hipp. They have determined to repeat the question. The answer to be given in before the 1st of November 1806.

4. They have received upon the question,—"What light has the new chemistry thrown upon the physiology of the human body?" a memoir in Dutch, having for motto, *Vita brevis, ars longa*, &c.; which was judged too superficial to receive the prize.

They have received also upon this question, and upon the two following:

5. "How far has the light thrown by the new chemistry upon the physiology of the human body contributed to improve the knowledge of certain diseases? and what useful consequences, in any degree confirmed by experience, can we deduce from it in the practice of medicine?"

6. "How far has the new chemistry tended to furnish precise notions with regard to the actions of certain external and internal remedies which have been either long in use or revived anew? and what advantages can we derive from a more exact knowledge of these actions in the treatment of certain diseases?" memoirs in French, having for motto, *Est modus in rebus*.

The society have determined to propose the following questions: candidates to deliver their answers by the 1st of November 1806:

"1. How far has chemistry explained the principles or component elements of plants, and more especially of such as furnish food? and what conclusions can we draw, either from the knowledge which we already possess upon this subject, or from what we can discover by experiment, aided by our knowledge of physiology, with regard to the vegetables which are best suited to the support of the human body both in health and in some diseases?"

"2. Are we enabled, by our present knowledge of the elements composing the food of animals, to explain satisfactorily the origin of the several principles or constituent parts of the human body; as for example, of the cal-

carcous earth, the soda, phosphorus, iron, &c.? or are these substances introduced into the animal body in some other way? or are there any experiments and observations which would lead us to suppose that at least some of those principles, although we are unable to compose or analyse them by chemical means, are produced by a peculiar action of the living organs?" In case the author should adopt in his answer the last opinion, it will be sufficient that he prove clearly the production of one of the above-mentioned principles.

"3. What are the facts which our experience has satisfactorily established regarding the acceleration of the germination of seeds, which Humboldt first attempted by sprinkling them with oxymuriatic acid:—as well as regarding the other methods, besides common manures and heat, which have been employed to forward the vegetation of plants in general and their germination in particular? how far can we explain, by the physiology of plants, in what manner these methods act? what assistance can we derive from our knowledge upon this point, in our further researches regarding either the means already had recourse to and others? and what advantage can we reap from the facts already ascertained on this subject, in the cultivation of useful vegetables?"

The following questions continue to be proposed for an unlimited time:

"1. What have we learnt by experience respecting the utility of certain animals which are apparently noxious, especially in the Low Countries? and what precautions are to be observed regarding their extirpation?"

"2. What are the indigenous plants hitherto least known for their virtues, which may be advantageously admitted into our dispensaries, and which may supply the place of foreign remedies?"

The memoirs which the society expects upon this question, must exhibit not only the virtues and advantages of these indigenous remedies, according to testimonies from foreign countries, but also according to proofs and observations made originally in our own provinces.

"3. What indigenous plants, hitherto not in use, can be employed for the purposes of wholesome nourishment at a low price? and what nutrient plants from other countries can be cultivated here with the same view?"

"4. What indigenous plants, hitherto not used as dyes, may be made to yield, according to accurate experiments, good colours, the preparation and use of which might be

introduced with advantage? and what foreign plants might be profitably cultivated in the more barren and waste lands of this republic, with a view to the extraction of dyes?"

The society repeat again, that according to the decree passed in the anniversary sitting of 1798, they will consider at each anniversary sitting the papers that have been communicated since the last meeting, which are not answers to questions proposed by the society; and that if there be found among them one treating of some branch of physics, or natural history, which merits a particular reward, they will adjudge to that memoir, or, if there be several of them, to the paper which shall appear most interesting, a silver medal having the usual device of the society, and in addition a gratification of ten ducats.

The answers may be made in Dutch, French, Latin, or German, but only in Roman characters: they must be accompanied by a sealed letter containing the name and address of the author, and sent to M. Van Marum, secretary to the society.

The prize destined for the memoir, which in the opinion of the society shall contain the best answer to any one of the questions above mentioned, is a gold medal, having the usual device of the society, and on the border the name of the author, with the year in which he received the prize, or thirty ducats, at the option of the person to whom the medal shall have been decreed.

LXX. *Intelligence and Miscellaneous Articles.*

VACCINATION.

By a calculation of ten years from January 1st 1791 to January 1st 1801, it appears that the average number of deaths at Vienna was 14,600, out of which were 835 children of the small-pox. In 1801, the period in which vaccination began to be introduced, out of 15,101 only 164 children fell victims to the small-pox: in 1802, out of 14,522 only 61: in 1803, the number out of 14,385 was only 27: and in 1804, out of 14,035, no more than 2.

At one of the late meetings of the original Vaccine Pock Institution, Broad-street, Golden-square, the subject of the antivaccine publications was discussed; and it was determined, that whatever unfortunate events might have occurred, the practice at this institution was uniformly as favourable, with regard to the fact of security against the small-pox, as could be expected from variolous inoculation.

 But

But in order to provoke the communication of failures, and to give a further proof to the public, of confidence, the following certificate was directed to be offered to each inoculated patient, deeming it just to employ the small funds of the institution in disabusing the public with regard to cow-pock inoculation, if adverse satisfactory evidence can be procured, or, if otherwise, in establishing the new practice.

Original Vaccine Pock Institution, Broad-street, Golden-Square, January 17, 1806.

This is to certify that E. C. aged two months, residing at K. has been duly vaccinated at this institution as reported in our register at No.

Be it known, that in case the aforesaid E. C. shall take the small-pox, the same being satisfactorily proved on examination of the disease by three or more of the medical officers of the institution, he shall be entitled to the sum of five guineas from the funds of the institution, on the presentation of this certificate; to be paid at the first quarterly court subsequently to the application.

Immediate notice must be sent to the secretary at the institution on the first appearance of the small-pox.

*** Preserve this testimonial for the purpose of claim.

George Pearson, M.D.
Thomas Nelson, M.D.
John Doratt, Surgeon.

ASTRONOMY.

Table of the right Ascension and Declination of Ceres and Juno, for February 1806.

	Ceres.		Juno.	
1806.	A.R. h m s	Dec. N. ° ′	A.R. h m s	Dec. ° ′
Feb. 2	6 53 16	31 6	11 42 40	1 18 S.
5	6 51 44	31 11	11 41 32	1 1
8	6 50 28	31 16	11 40 12	0 41
11	6 49 32	31 19	11 38 40	0 19
14	6 48 52	31 22	11 37 0	0 4 N.
17	6 28 28	31 24	11 35 8	0 28
20	6 28 24	31 25	11 33 8	0 54
23	6 28 40	31 26	11 31 4	1 21
26	6 29 8	31 26	11 28 52	1 48

Pallas is too low to be seen.

COMET.

COMET.

On the 22d of October, M. Huth, of Frankfort on the Oder, at three o'clock in the morning, discovered a comet in the hindermost foot of the Great Bear, betwixt the stars ν and ξ. It could hardly be seen with the naked eye, but was very visible with a common telescope. In size and brightness it resembled the great nebulous spot in Andromeda, except that it was almost circular. At four A. M. its right ascension was about 166° 30′, with 30° 40′ declination north; and in an hour after, the former was 166° 32′, and the latter 33° 32′. Its course is therefore southerly, and somewhat westerly. When magnified 350 times, it did not show any nucleus.

The same comet was discovered on the following day by professor Bode, at Berlin, between two and three o'clock in the morning, at ⊛ of the Great Bear. Right ascension 174° 25′, and 270° 40′ north declination.

WOOD.

The following particulars are transcribed from a letter of John Templeton, esq. published in the third volume of the Transactions of the Dublin Society:

"It is said that there has been no timber found at Botany Bay but what sinks in water. The Robinia Pseudo-Acacia, or locust-tree of North America, is esteemed beyond all others for the durability of its timber, when exposed to the weather, and for making trennels or wooden pins for fastening the planks of ships. Luckily in light soils it thrives well in Ireland, and sends forth suckers by which it is easily propagated. It grows with great quickness; I have had some, which made shoots five and six feet in a season, in a sandy soil. They are not injured by our severest frosts; but care must be taken to place them in such situations as that they will not be exposed to high winds, which often break their branches.

The Fagus Castanea, or sweet chesnut, is among our most beautiful trees of quick growth in a sandy or light loamy soil. There are four on my farm of considerable size; one is above eleven feet in girt, growing rapidly; and yet there is reason to believe them about a hundred years old. The timber is esteemed the best of European growth, for a great variety of uses. In the southern parts it is preferred for wine casks, as it is not apt to warp or shrink. In England it is reckoned the best for hop-poles, as even young poles stand wet and dry better than any other. Its durability has been pretty well determined by an experiment related in Young's Annals of Agriculture.

"Inch and half planks of trees from thirty to forty-five

years growth, after ten years standing in the weather, were examined, and found in the following state and condition :

Cedar, perfectly sound.
Larch, the heart sound, but sap quite decayed.
Spruce fir, sound.
Silver fir, in decay.
Scotch fir, much decayed.
Pinaster, quite rotten.
Chesnut, perfectly sound.
Abele, sound.
Beech, sound.
Walnut, in decay.
Sycamore, much decayed.
Birch, quite rotten.

Young's Annals of Agriculture, vol. vi. 256.

A piece of a branch cut off, and put in as a gate-post twenty-five years ago, is yet perfectly sound, even at the surface of the ground, a time that, I believe, would have rotted the best oak. Another property which recommends chesnut is, that it stains well, with a decoction of logwood and Brazil-wood, with alum water, varying the proportions at will. I have imitated mahogany so as to deceive most people.

MEDICAL AND CHEMICAL LECTURES.

On Thursday, February 6th, a Course of Lectures on Physic and Chemistry will recommence at the Laboratory, Whitcomb-street, Leicester-square, at the usual morning hours, by George Pearson, M.D. F.R.S. &c.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Fletcher, of Horsley, in the county of Derby, needle-maker; for a machine for raising water. Dated January 23, 1806.

To George Barton Alcock, of the city of Kilkenny, in that part of the united kingdom of Great Britain and Ireland called Ireland; for certain improvements in lamps. Dated January 22.

To John Dobbs Davies, of New Compton-street, in the county of Middlesex, gentleman; for a saddle bar on an improved construction, which he denominates the *motion saddle bar*. Dated January 22.

To Robert Berriman, of Speen, in the county of Berks, wheelwright; for a machine for preparing land for the reception of seed, which he is confident will prove of the utmost advantage to agriculturists in sowing corn, in producing a regular and more abundant crop, and in enabling the farmer, at an easier rate, to keep his land free from all kinds of weeds. Dated January 22.

METEOR-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For January 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec. 27	34°	40°	32°	29·87	7°	Fair
28	34	40	44	·80	0	Rain
29	47	51	49	·82	0	Rain
30	51	52	53	·89	0	Rain
31	53	53	49	·82	5	Cloudy
Jan. 1	43	43	48	·70	5	Cloudy
2	36	39	35	·80	8	Fair
3	35	38	32	·82	6	Fair
4	33	41	49	·80	0	Rain
5	40	44	49	·88	12	Fair
6	49	53	46	·85	9	Cloudy
7	39	48	48	30·00	0	Rain
8	47	49	38	29·50	20	Fair
9	39	44	40	·57	18	Fair
10	36	41	35	28·72	17	Fair; a storm in the morning, with thunder
11	37	41	36	29·10	20	Fair
12	36	39	36	28·65	0	Rain
13	36	39	33	29·65	10	Fair
14	40	43	39	·60	12	Fair
15	41	45	45	·49	22	Fair
16	50	46	40	·01	0	Stormy
17	36	44	33	·70	21	Fair
18	38	44	49	·85	0	Rain
19	43	49	44	·68	24	Fair
20	51	54	47	·52	0	Rain
21	50	55	46	·82	15	Fair
22	49	53	46	·92	7	Cloudy
23	59	50	47	·90	23	Cloudy
24	48	51	40	·52	21	Fair
25	39	44	37	·60	24	Fair
26	35	41	36	·25	4	Cloudy

N. B. The barometer's height is taken at noon.

INDEX TO VOL. XXIII.

Candles.

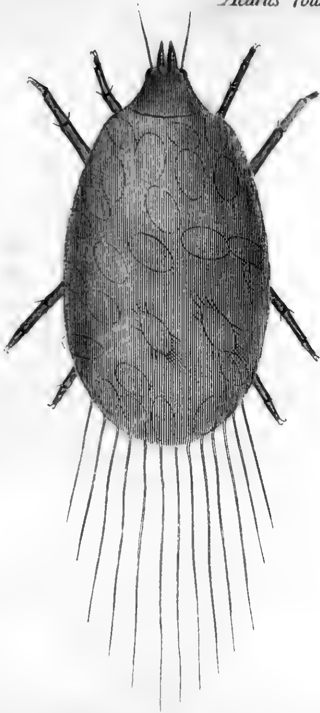
Muscular

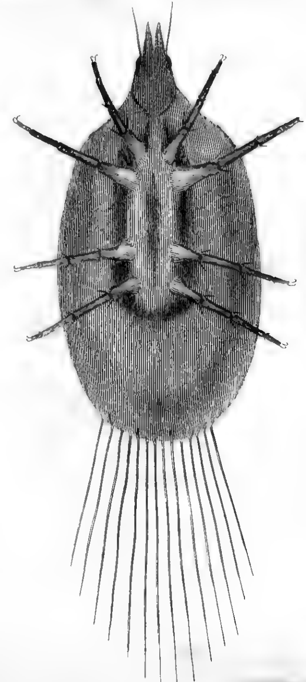
Tinea

END OF THE TWENTY-THIRD VOLUME.

Printed by R. Taylor and Co., 38, Shoe Lane, Fleet Street.

Acarus found on the Wings &c of a feathered Fly.





Analysis of Soils, by H. Davy Esq.

Lowry sculp.

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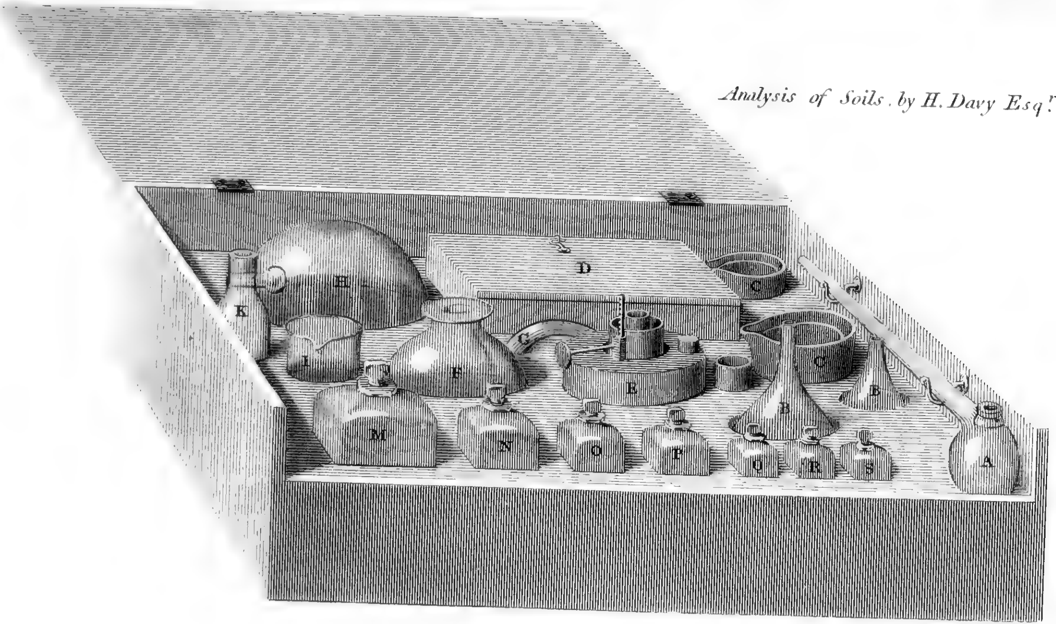
Analysis of Soils, by H. Davy Esq.r



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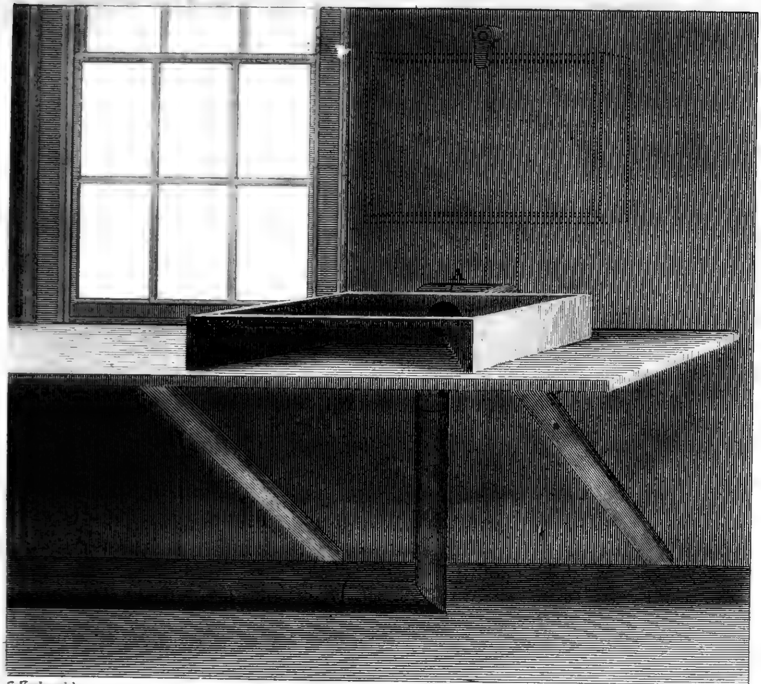
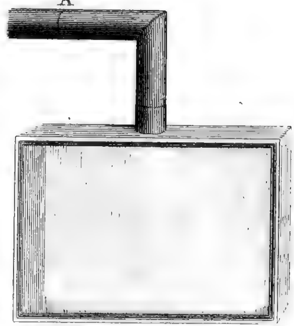


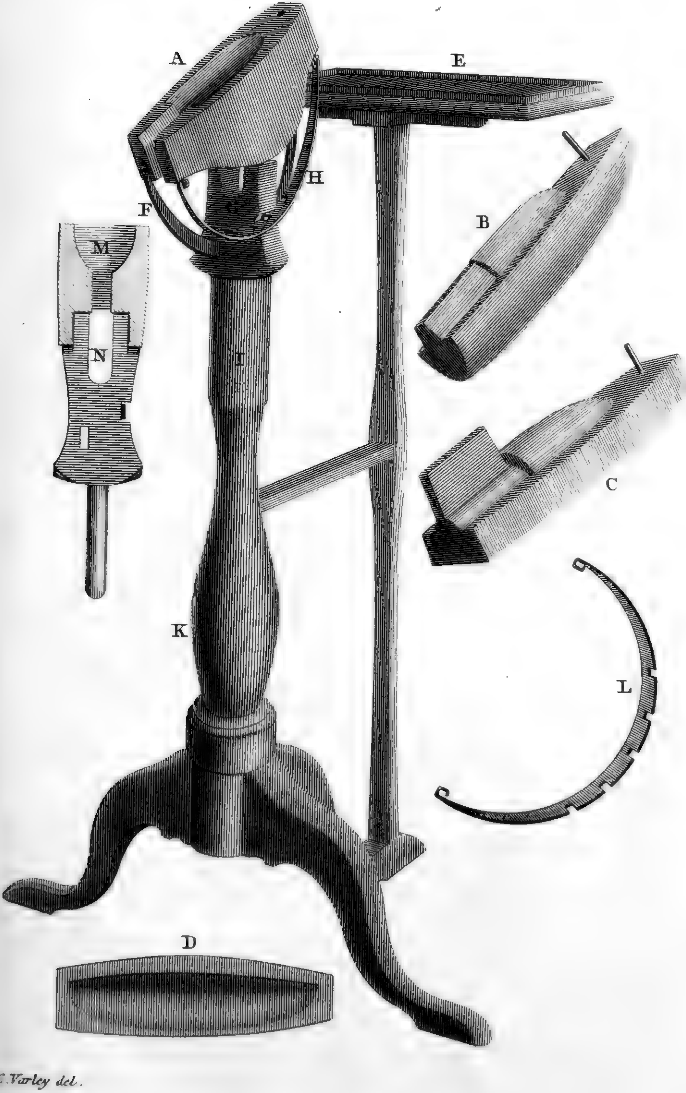


C. Varley del.

Lowry sculp.

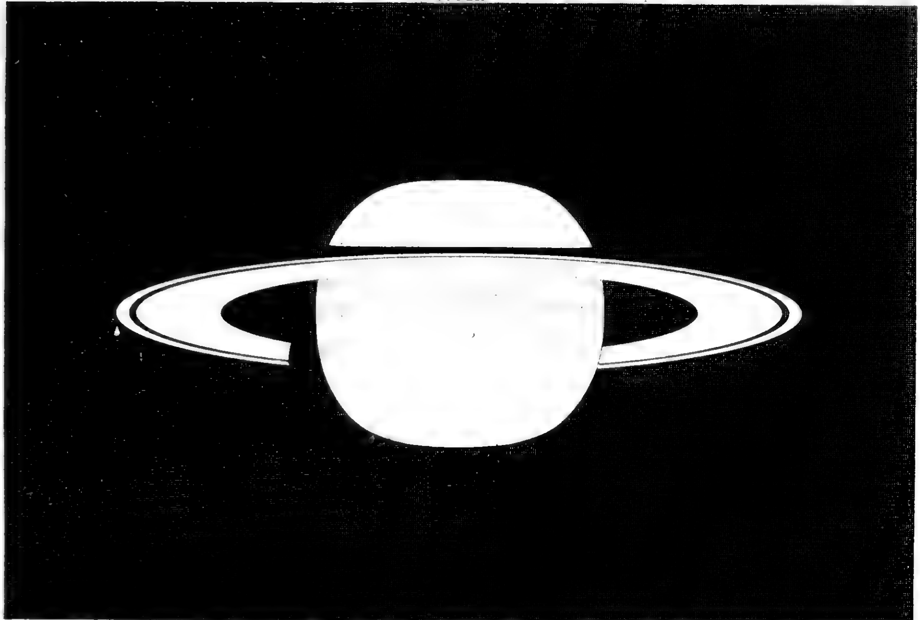
Skeleton of an Ibis, from a Mummy at Thebes.

Mr. Holden's Machine for Shoe makers.



C. Varley del.

SOUTH

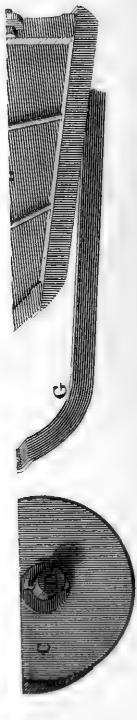


NORTH

Engraved by Lowry

Syst

Fig. 3.



Lowry sculp.

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Lyra
Capella
Arcturus

Mr. David Charles's Machine
for laying Land level.

Fig. 2.

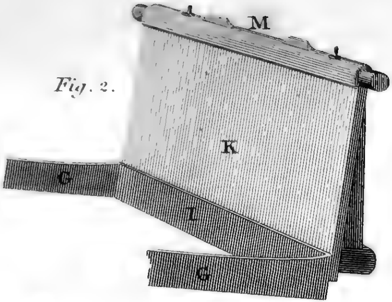
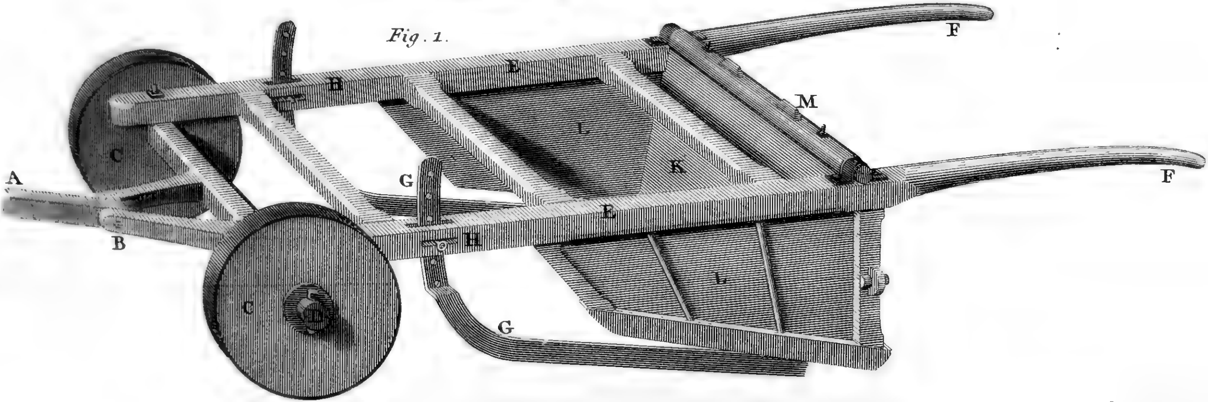


Fig. 1.



Lowry sculp.

Apparatus for preparing Gaseous Oxide of Carbon.